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CULTURAL ANTHROPOLOGY AND PSYCHIATRY *

By E. SAPIR
YALE UNIVERSITY

BEFORE we try to establish a more intimate relation between the problems of cultural anthropology and those of psychiatry than is generally recognized, it will be well to emphasize the apparent differences of subject matter and purpose which seem to separate them as disciplines concerned with human behavior. In the main, cultural anthropology has emphasized the group and its traditions in contradistinction to individual variations of behavior. It aims to discover the generalized forms of action, thought and feeling which, in their complex interrelatedness, constitute the culture of a community. Whether the ultimate aim of such a study is to establish a typical sequence of institutional forms in the history of man, or to work out a complete distributional survey of patterns and cultural types over the globe, or to make an exhaustive descriptive analysis of as many cultures as possible in order that fundamental sociological laws may be arrived at, is important, indeed, for the spirit and method of actual research in the field of human culture. But all these approaches agree in thinking of the individual as a more or less passive carrier of tradition or, to speak more dynamically, as the infinitely variable actualizer of ideas and of modes of behavior which are implicit in the structure and tradition of a given society. It is what all the individuals of a society have in common in their mutual relations which is supposed to constitute the true subject matter of cultural anthropology and sociology. If the testimony of an individual is set down as such, as often happens in our anthropological monographs, it is not because of an interest in the individual himself as a matured and single organism of ideas but in his assumed typicality for the community as a whole.

* Manuscript received October 3, 1932.

It is true that there are many statements in our ethnological monographs which, for all that they are presented in general terms, really rest on the authority of a few individuals, or even of one individual, who have had to bear testimony for the group as a whole. Information on kinship systems or rituals or technological processes or details of social organization or linguistic forms is not ordinarily evaluated by the cultural anthropologist as a personal document. He always hopes that the individual informant is near enough to the understandings and intentions of his society to report them duly, thereby implicitly eliminating himself as a factor in the method of research. All realistic field workers in native custom and belief are more or less aware of the dangers of such an assumption and, naturally enough, efforts are generally made to "check up" statements received from single individuals. This is not always possible, however, and so our ethnological monographs present a kaleidoscopic picture of varying degrees of generality, often within the covers of a single volume. Thus, that the Haida Indians of Queen Charlotte Islands were divided into two exogamic phratries, the Eagles and the Ravens, is a statement which could, no doubt, be elicited from any normal Haida Indian. It has very nearly the same degree of impersonality about it that characterizes the statement that the United States is a republic governed by a President. It is true that these data about social and political organization might mean rather different things in the systems of ideas and fantasies of different individuals or might, as master ideas, be construed to lead to typically different forms of action according to whether we studied the behavior of one individual or of another. But that is another matter. The fundamental patterns are relatively clear and impersonal. Yet in many cases we are not so fortunate as in the case of fundamental outlines of political organization or of kinship terminology or of house structure. What shall we do, for instance, with the cosmogonic system of the Bella Coola Indians of British Columbia? The five superimposed worlds which we learn about in this system not only have no close parallels among the other tribes of the Northwest Coast area but have not been vouched for by any informant other than the one individual from whom Boas obtained his information. Is this cosmogonic system typical Bella Coola religious belief? Is it individual fantasy construction or is it a peculiar individual elaboration on the basis of a simpler cosmogonic system which belongs to the community as a whole? In this special instance the individual note obtrudes itself somewhat embarrassingly. In the main, however, the

cultural anthropologist believes or hopes that such disquieting interruptions to the impersonality of his thinking do not occur frequently enough to spoil his science.

Psychiatry is an offshoot of the medical tradition and aims to diagnose, analyze, and, if possible, cure those behavior disturbances of individuals which show to observation as serious deviations from the normal attitude of the individual toward his physical and social environment. The psychiatrist specializes in "mental" diseases as the dermatologist specializes in the diseases of the skin or the gynecologist concerns himself with diseases peculiar to women. The great difference between psychiatry and the other biologically defined medical disciplines is that while the latter have a definite bodily locus to work with and have been able to define and perfect their methods by diligent exploration of the limited and tangible area of observation assigned to them, psychiatry is apparently doomed to have no more definite locus than the total field of human behavior in its more remote or less immediately organic sense. The conventional companionship of psychiatry and neurology seems to be little more than a declaration of faith by the medical profession that all human ills are, at last analysis, of organic origin and that they are, or should be, localizable in some segment, however complexly defined, of the physiological machine. It is an open secret, however, that the neurologist's science is one thing and the psychiatrist's practice another. Almost in spite of themselves psychiatrists have been forced to be content with an elaborate array of clinical pictures, with terminological problems of diagnosis, and with such thumb rules of clinical procedure as seem to offer some hope of success in the handling of actual cases. It is no wonder that psychiatry tends to be distrusted by its sister disciplines within the field of medicine and that the psychiatrists themselves, worried by a largely useless medical training and secretly exasperated by their inability to apply the strictly biological part of their training to their peculiar problems, tend to magnify the importance of the biological approach in order that they may not feel that they have strayed away from the companionship of their more illustrious brethren. No wonder that the more honest and sensitive psychiatrists have come to feel that the trouble lies not so much in psychiatry itself as in the rôle which general medicine has wished psychiatry to play.

These insurgent psychiatrists, among whom Freud must be reckoned the most courageous and the most fertile in ideas, have come to feel that many of the so-called nervous and mental dis-

orders can be looked upon as the logical development of systems of ideas and feelings which have grown up in the experience of the individual and which have an unconscious value for him as the symbolic solution of profound difficulties that arise in an effort to adjust to his human environment. The morbidity, in other words, that the psychiatrist has to deal with seems, for the most part, to be not a morbidity of organic segments or even of organic functions but of experience itself. His attempts to explain a morbid suspiciousness of one's companions or delusion as to one's status in society by some organically definable weakness of the nervous system or of the functioning of the endocrine glands may be no more to the point than to explain the habit of swearing by the absence of a few teeth or by a poorly shaped mouth. This is not the place to go into an explanation, however brief, of the new points of view which are to be credited to Freud and his followers and which have invaded the thinking of even the most conservative of psychiatrists to no inconsiderable extent. All that interests us here is to note the fact that psychiatry is moving away from its historic position of a medical discipline that is chronically unable to make good to that of a discipline that is medical only by tradition and courtesy and is compelled, with or without permission, to attack fundamental problems of psychology and sociology so far as they affect the well-being of the individual. The locus, then, of psychiatry turns out not to be the human organism at all in any fruitful sense of the word but the more intangible, and yet more intelligible, world of human relationships and ideas that such relationships bring forth. Those students of medicine who see in these trends little more than a return to the old mythology of the "soul" are utterly unrealistic, for they tacitly assume that all experience is but the mechanical sum of physiological processes lodged in isolated individuals. This is no more defensible a position than the naïvely metaphysical contention that a table or chair or hat or church can be intelligibly defined in terms of their molecular and atomic constitution. That A hates B or hopelessly loves B or is jealous of B or is mortally afraid of B or hates him in one respect and loves him in another can result only from the complications of experience. If we work out a gradually complicating structure of morbid relationships between A and B and, by successive transfers, between A or B and the rest of the human world, we discover behavior patterns that are none the less real and even tragic for not being fundamentally attributable to some weakness or malfunctioning of the nervous system or any other part of the organism. This does not mean that weakness or mal-

functioning of a strictly organic character may not result from a morbidity of human relationships. Such an organic theory would be no more startling than to maintain that a chronic sneer may disfigure the shape of the mouth or that a secret fear may impair one's digestion. There are, indeed, signs that psychiatry, slowly and painfully delivering itself from the somatic superstitions of medicine, may take its revenge by attempts to "mentalize" large sections of medical theory and practice. The future alone can tell how much of these psychological interpretations of organic disease is sound doctrine or a new mythology.

There is reason, then, to think that while cultural anthropology and psychiatry have distinct problems to begin with, they must, at some point, join hands in a highly significant way. That culture is a superorganic, impersonal whole is a useful enough methodological principle to begin with but becomes a serious deterrent in the long run to the more dynamic study of the genesis and development of cultural patterns because these cannot be realistically disconnected from those organizations of ideas and feelings which constitute the individual. The ultimate methodological error of the student of personality is perhaps less obvious than the correlative error of the student of culture but is all the more insidious and dangerous for that reason. Mechanisms which are unconsciously evolved by the neurotic or psychotic are by no means closed systems imprisoned within the biological walls of isolated individuals. They are tacit commentaries on the validity or invalidity of some of the more intimate implications of culture for the adjustment processes of given individuals. We are not, therefore, to begin with a simple contrast between social patterns and individual behavior, whether normal or abnormal, but we are, rather, to ask what is the meaning of culture in terms of individual behavior and whether the individual can, in a sense, be looked upon as the effective carrier of the culture of his group. As we follow tangible problems of behavior rather than the selected problems set by recognized disciplines, we discover the field of social psychology, which is not a whit more social than it is individual and which is, or should be, the mother science from which stem both the abstracted impersonal problems as phrased by the cultural anthropologist and the almost impertinently realistic explorations into behavior which are the province of the psychiatrist. Be it remarked in passing that what passes for individual psychology is little more than an ill-assorted mélange of bits of physiology and of studies of highly fragmentary modes of behavior which have been artificially induced by the psychologist. This abortive dis-

cipline seems to be able to arrive at no integral conceptions of either individual or society and one can only hope that it will eventually surrender all its problems to physiology and social psychology.

Cultural anthropology has not been neglected by psychiatry. The psychoanalysts in particular have made very extensive use of the data of cultural anthropology in order to gather evidence in support of their theories of the supposed "racial inheritance of ideas" by the individual. Neurotic and psychotic, through the symbolic mechanisms which control their thinking, are believed to regress to a more primitive state of mental adjustment than is normal in modern society and which is supposed to be preserved for our observation in the institutions of primitive peoples. In some undefined way which it seems quite impossible to express in intelligible biological or psychological terms the cultural experiences which have been accumulated by primitive man are believed to be unconsciously handed on to his more civilized progeny. The resemblances between the content of primitive ritual—and symbolic behavior generally among primitive peoples—and the apparently private rituals and symbolisms developed by those who have greater than normal difficulty in adjusting to their social environment are said to be so numerous and far-reaching that the latter must be looked upon as an inherited survival of more archaic types of thought and feeling. Hence, we are told, it is very useful to study the culture of primitive man, for in this way an enormous amount of light is thrown upon the fundamental significance of modes of behavior in the neurotic which are otherwise inexplicable. The searching clinical investigation into the symbolisms of the neurotic recovers for us, on a modern and highly disguised level, what lies but a little beneath the surface among the primitives, who are still living under an archaic psychological régime.

Psychoanalysts welcome the contributions of cultural anthropology but it is exceedingly doubtful if many cultural anthropologists welcome the particular spirit in which the psychoanalysts appreciate their data. The cultural anthropologist can make nothing of the hypothesis of the racial unconscious nor is he disposed to allow an immediate psychological analysis of the behavior of primitive people in any other sense than that in which such an analysis is allowable for our own culture. He believes that it is as illegitimate to analyze totemism or primitive laws of inheritance or set rituals in terms of the peculiar symbolisms discovered or invented by the psychoanalyst as it would be to analyze the most complex forms of modern social behavior in these terms. And

he is disposed to think that if the resemblances between the neurotic and the primitive which have so often been pointed out are more than fortuitous it is not because of a cultural atavism which the neurotic exemplifies but simply because all human beings, whether primitive or sophisticated in the cultural sense, are, at rock bottom, psychologically primitive, and there is no reason why a significant unconscious symbolism which gives substitutive satisfaction to the individual may not become socialized on any level of human activity.

The service of cultural anthropology to psychiatry is not as mysterious or remote or clandestine as psychoanalytic mysticism would have us believe. It is of a much simpler and healthier sort. It lies very much nearer the surface of things than is generally believed. Cultural anthropology, if properly understood, has the healthiest of all scepticisms about the validity of the concept "normal behavior". It cannot deny the useful tyranny of the normal in a given society but it believes the external form of normal adjustment to be an exceedingly elastic thing. It is very doubtful if the normalities of any primitive society that lies open to inspection are nearer the hypothetical responses of an archaic type of man, untroubled by a burdensome historical past, than the normalities of a modern Chinese or Scotchman. In specific instances one may even wonder whether they are not tangibly less so. It would be more than a joke to turn the tables and to suggest that the psychoanalysis of an over-ritualized Pueblo Indian or Toda might denude him sufficiently to set him "regressing" to the psychologically primitive status of an American professor's child or of a professor himself. The cultural anthropologist's quarrel with psychoanalysis can perhaps be put most significantly by pointing out that the psychoanalyst has confused the archaic in the conceptual or theoretical psychologic sense with the archaic in the literal chronological sense. Cultural anthropology is not valuable because it uncovers the archaic in the psychological sense. It is valuable because it is constantly rediscovering the normal. For the psychiatrist and for the student of personality in general this is of the greatest importance, for personalities are not conditioned by a generalized process of adjustment to "the normal" but by the necessity of adjusting to the greatest possible variety of idea patterns and action patterns according to the accidents of birth and biography.

The so-called culture of a group of human beings, as it is ordinarily treated by the cultural anthropologist, is essentially a systematic list of all the socially inherited patterns of behavior

which may be illustrated in the actual behavior of all or most of the individuals of the group. The true locus, however, of these processes which, when abstracted into a totality, constitute culture is not in a theoretical community of human beings known as society, for the term "society" is itself a cultural construct which is employed by individuals who stand in significant relations to each other in order to help them in the interpretation of certain aspects of their behavior. The true locus of culture is in the interactions of specific individuals and, on the subjective side, in the world of meanings which each one of these individuals may unconsciously abstract for himself from his participation in these interactions. Every individual is, then, in a very real sense, a representative of at least one sub-culture which may be abstracted from the generalized culture of the group of which he is a member. Frequently, if not typically, he is a representative of more than one sub-culture, and the degree to which the socialized behavior of any given individual can be identified with or abstracted from the typical or generalized culture of a single group varies enormously from person to person.

It is impossible to think of any cultural pattern or set of cultural patterns which can, in the literal sense of the word, be referred to society as such. There are no facts of political organization or family life or religious belief or magical procedure or technology or aesthetic endeavor which are coterminous with society or with any mechanically or sociologically defined segment of society. The fact that John Doe is registered in some municipal office as a member of such and such a ward only vaguely defines him with reference to those cultural patterns which are conveniently assembled under some such term as "municipal administration". The psychological and, in the deepest sense of the word, the cultural realities of John Doe's registration may, and do, vary enormously. If John Doe is paying taxes on a house which is likely to keep him a resident of the ward for the rest of his life and if he also happens to be in personal contact with a number of municipal officers, ward classification may easily become a symbol of his orientation in his world of meanings which is comparable for clarity, if not for importance, to his definition as a father of a family or as a frequent participant in golf. Ward membership, for such an individual, may easily precipitate itself into many visible forms of behavior. The ward system and its functions, real or supposed, may for such a John Doe assume an impersonal and objective reality which is comparable to the objective reality of rain or sunshine.

But there is sure to be another John Doe, perhaps a neighbor of the first, who does not even know that the town is divided into wards and that he is, by definition, enrolled in one of them and that he has certain duties and privileges connected with such enrollment, whether he cares to exercise them or not. While the municipal office classifies these two John Does in exactly the same way and while there is a theory on foot that ward organization, with its associated functions, is an entirely impersonal matter to which all members of a given society must adjust, it is rather obvious that such a manner of speech is little more than a sociological metaphor. The cultures of these two individuals are, as a matter of fact, significantly different, as significantly different, on the given level and scale, as though one were the representative of Italian culture and the other of Turkish culture. Such differences of culture never seem as significant as they really are; partly because in the workaday world of experience they are not often given the opportunity to emerge into sharp consciousness, partly because the economy of interpersonal relations and the friendly ambiguities of language conspire to reinterpret for each individual all behavior which he has under observation in the terms of those meanings which are relevant to his own life. The concept of culture, as it is handled by the cultural anthropologist, is necessarily something of a statistical fiction and it is easy to see that the social psychologist and the psychiatrist must eventually induce him to carefully reconsider his terms. It is not the concept of culture which is subtly misleading but the metaphysical locus to which culture is generally assigned.

Clearly, not all cultural traits are of equal importance for the development of personality, for not all of them are equally diffused as integral elements in the idea-systems of different individuals. Some modes of behavior and attitude are pervasive and compelling beyond the power of even the most isolated individual to withstand or reject. Such patterns would be, for example, the symbolisms of affection or hostility; the overtones of emotionally significant words; certain fundamental implications and many details of the economic order; much, but by no means all, of those understandings and procedures which constitute the law of the land. Patterns of this kind are compulsive for the vast majority of human beings but the degree of compulsiveness is in no simple relation to the official, as contrasted with the inner or psychological, significance of these patterns. Thus, the use of an offensive word may be of negligible importance from a legal standpoint but may, psychologically considered, have an attracting or repelling

potency that far transcends the significance of so serious a behavior pattern as, say, embezzlement or the nature of one's scientific thinking. A culture as a whole cannot be said to be adequately known for purposes of personality study until the varying degrees of compulsiveness which attach to its many aspects and implications are rather definitely understood. No doubt there are cultural patterns which tend to be universal, not only in form but in psychological significance, but it is very easy to be mistaken in these matters and to impute equivalences of meaning which do not truly exist.

There are still other cultural patterns which are real and compelling only for special individuals or groups of individuals and are as good as non-existent for the rest of the group. Such, for instance, are the ideas, attitudes and modes of behavior which belong to specialized trades. We are all aware of the reality of such private or limited worlds of meaning. The dairyman, the movie actress, the laboratory physicist, the party whip, have obviously built up worlds which are anonymous or opaque to each other or, at best, stand to each other in a relation of blanket acceptance. There is much tacit mythology in such hugely complex societies as our own which makes it possible for the personal significance of sub-cultures to be overlooked. For each individual, the commonly accepted fund of meanings and values tends to be powerfully specialized or emphasized or contradicted by types of experience and modes of interpretation that are far from being the property of all men. If we consider that these specialized cultural participations are partly the result of contact with limited traditions and techniques, partly the result of identification with such biologically and socially imposed groups as the family or the class in school or the club, we can begin to see how inevitable it is that the true psychological locus of *a* culture is *the individual* or *a specifically enumerated list of individuals,* not an economically or politically or socially defined group of individuals. "Individual", however, here means not simply a biologically defined organism maintaining itself through physical impacts and symbolic substitutes of such impacts, but that total world of form, meaning and implication of symbolic behavior which a given individual partly knows and directs, partly intuits and yields to, partly is ignorant of and is swayed by.

Still other cultural patterns have neither a generalized nor a specialized potency. They may be termed marginal or referential and while they may figure as conceptually important in the schema of a cultural theorist, they may actually have little or no psychological importance for the normal human being. Thus, the force

of linguistic analogy which creates the plural "unicorns" is a most important force for the linguistic analyst to be clear about, but it is obvious that the psychological imminence of that force, while perfectly real, may be less than the avoidance, say, of certain obscene or impolite words, an avoidance which the linguist, in turn, may quite legitimately look upon as marginal to his sphere of interests. In the same way, while such municipal subdivisions as wards are, from the standpoint of political theory, of the same order as state lines and even national lines, they are not psychologically so. They are psychologically related to such saturated entities as New York or "the South" or Fifth Avenue or "the slums" as undeveloped property in the suburbs is economically related to real estate in the business heart of a great metropolis. Some of this marginal cultural property is held as marginal by the vast majority of participants in the total culture, if we may still speak in terms of a "total culture". Others of these marginal patterns are so only for certain individuals or groups of individuals. No doubt, to a movie actress the intense world of values which engages the participation of a physicist tends to be marginal in about the same sense as a legal fiction or unactualized linguistic possibility may be marginal cultural property. A "hard-headed business man" may consign the movie actress and the physicist to two adjoining sectors, "lively" and "sleepy" respectively, of a marginal tract of "triviality". Culture, then, varies infinitely, not only as to manifest content but as to the distribution of psychologic emphases on the elements and implications of this content. According to our scale of treatment, we have to deal with the cultures of groups and the cultures of individuals.

A personality is carved out by the subtle interaction of those systems of ideas which are characteristic of the culture as a whole, as well as of those systems of ideas which get established for the individual through more special types of participation, with the physical and psychological needs of the individual organism, which cannot take over any of the cultural material that is offered in its original form but works it over more or less completely, so that it integrates with those needs. The more closely we study this interaction, the more difficult it becomes to distinguish society as a cultural and psychological unit from the individual who is thought of as a member of the society to whose culture he is required to adjust. No problem of social psychology that is at all realistic can be phrased by starting with the conventional contrast of the individual and his society. Nearly every problem of social psychology needs to consider the exact nature and implication of an

idea complex, which we may look upon as the psychological correlate of the anthropologist's cultural pattern, to work out its relation to other idea complexes and what modifications it necessarily undergoes as it accommodates itself to these, and, above all, to ascertain the precise locus of such a complex. This locus is rarely identifiable with society as a whole, except in a purely philosophical or conceptual sense, nor is it often lodged in the psyche of a single individual. In extreme cases such an idea complex or cultural pattern may be the dissociated segment of a single individual's mind or it may amount to no more than a potential revivification of ideas in the mind of a single individual through the aid of some such symbolic depositary as a book or museum. Ordinarily the locus will be a substantial portion of the members of a community, each of them feeling that he is touching common interests so far as this particular culture pattern is concerned. We have learned that the individual in isolation from society is a psychological fiction. We have not had the courage to face the fact that formally organized groups are equally fictitious in the psychological sense, for geographically contiguous groups are merely a first approximation to the infinitely variable groupings of human beings to whom culture in its various aspects is actually to be credited as a matter of realistic psychology.

"Adjustment", as the term is ordinarily understood, is a superficial concept because it regards only the end product of individual behavior as judged from the standpoint of the requirements, real or supposed, of a particular society. In reality "adjustment" consists of two distinct and even conflicting types of process. It includes, obviously, those accommodations to the behavior requirements of the group without which the individual would find himself isolated and ineffective, but it includes, just as significantly, the effort to retain and make felt in the opinions and attitudes of others that particular cosmos of ideas and values which has grown up more or less unconsciously in the experience of the individual. Ideally these two adjustment tendencies need to be compromised into behavior patterns which do justice to both requirements.

It is a dangerous thing for the individual to give up his identification with such cultural patterns as have come to symbolize for him his own personality integration. The task of external adjustment to social needs may require such abandonment on his part and consciously he may crave nothing more passionately, but if he does not wish to invite disharmony and inner weakness in his personality, he must see to it, consciously or unconsciously, that every abandonment is made good by the acquisition of a psycho-

logically equivalent symbolism. External observations on the adjustment processes of individuals are often highly misleading as to their psychological significance. The usual treatment, for instance, of behavior tendencies known as radical and conservative must leave the genuine psychiatrist cold because he best realizes that the same types of behavior, judged externally, may have entirely distinct, even contradictory, meanings for different individuals. One may be a conservative out of fear or out of superb courage. A radical may be such because he is so secure in his fundamental psychic organization as to have no fear for the future, or, on the contrary, his courage may be merely the fantasied rebound from fear of the only too well known.

Strains which are due to this constant war of adjustment are by no means of equal intensity for all individuals. Systems of ideas grow up in endless ways, both within a so-called uniform culture and through the blending of various aspects of so-called distinct cultures, and very different symbolisms and value emphases necessarily arise in the endless sub-cultures or private symbol organizations of the different members of a group. This is tantamount to saying that certain systems of ideas are more perilously exposed to the danger of disintegration than others. Even if it be granted, as no one would seriously argue that it should not, that individual differences of an inherited sort are significantly responsible for mental breakdowns, it yet remains true that such a "failure" in the life of an individual cannot be completely understood by the study, however minute, of the individual's body and mind as such. Such a failure invites a study of his system of ideas as a more or less distinct cultural entity which has been vainly striving to maintain itself in a discouraging environment.

We may go so far as to suggest quite frankly that a psychosis, for instance, may be an index at one and the same time of the too great resistance of the individual to the forces that play upon him and, so far as *his* world of values is concerned, of the cultural poverty of his psychological environment. The more obvious conflicts of cultures with which we are familiar in the modern world create an uneasiness which forms a fruitful soil for the eventual development, in particular cases, of neurotic symptoms and mental breakdowns but they can hardly be considered sufficient to account for serious psychological derangements. These arise not on the basis of a generalized cultural conflict but out of specific conflicts of a more intimate sort, in which systems of ideas get attached to particular persons, or images of such persons, who

play a decisive rôle in the life of the individual as representatives of cultural values.

The personal meaning of the symbolisms of an individual's subculture are constantly being reaffirmed by society or, at the least, he likes to think that they are. When they obviously cease to be, he loses his orientation and that strange instinct, or whatever we call it, which in the history of culture has always tended to preserve a system of ideas from destruction, causes his alienation from an impossible world. Both the psychosis and the development of an idea or institution through the centuries manifest the stubbornness of idea complexes and their implications in the face of a material environment which is less demanding psychologically than physically. The mere problem of biological adjustment, or even of ego adjustment as it is ordinarily handled by the sociologist, is comparatively simple. It is literally true that "man wants but little here below nor wants that little long". The trouble always is that he wants that little on his own terms. It is not enough to satisfy one's material wants, to have success in one's practical endeavors, to give and receive affection, or to accomplish any of the purposes laid down by psychologists and sociologists and moralists. Personality organizations, which at last analysis are psychologically comparable with the greatest cultures or idea systems, have as their first law of being their essential self-preservation, and all conscious attempts to define their functions or to manipulate their intention and direction are but the estimable rationalization of people who are wanting to "do things". Modern psychiatrists should be tolerant not only of varying personalities but of the different types of values which personality variations imply. Psychiatrists who are tolerant only in the sense that they refrain from criticizing anybody who is subjected to their care and who do their best to guide him back to the renewed performance of society's rituals may be good practical surgeons of the psyche. They are not necessarily the profoundly sympathetic students of the mind who respect the fundamental intent and direction of every personality organization.

Perhaps it is not too much to expect that a number of gifted psychiatrists may take up the serious study of exotic and primitive cultures, not in the spirit of meretricious voyaging in behalf of Greenwich Village nor to collect an anthology of psychoanalytic fairy tales, but in order to learn to understand, more fully than we can out of the resources of our own cultures, the development of ideas and symbols and their relevance for the problem of personality.

SOCIAL PARTICIPATION AMONG PRE-SCHOOL CHILDREN

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GENETIC Sociology is as yet a little developed field of science. Investigators of social behavior have overlooked the period when adjustment to the group is first acquired and praticed. In so doing, they have ignored a source that ought to contribute not only to the explanation of child behavior, but to the understanding of adult group habits which persist from childhood. By the time individuals have acquired their "human nature", their overt group responses are determined by such a multiplicity of factors such as customs, mores, traditions, social controls, past experience in groups, emotions and native equipment, that many scientists have despaired of ever finding any uniformities in their behavior. The genetic approach to the study of social motivation and adjustment promises to reveal group behavior which is only slightly affected by these complex social factors. The reactions of children are more or less spontaneous and overt and therefore perceptible to investigators.

Most of the investigations on the genesis of social behavior that have been made up to the present time are to be found in the psychological and educational rather than in the sociological literature.

Throughout the literature which deals with the subject of the behavior of young children are scattered varied and often contradictory theories and generalizations regarding social development, traits, and habits. In the first place, there is no uniformity in the use and meaning of the term "social". Very often a moral or ethical connotation is expressed or implied. Sometimes altruistic behavior is regarded as synonymous with social. Then again, self-sufficiency in group life is conceived of as "social behavior". Occasionally, "social" is employed to denote "play" behavior. More recent scientific approaches usually use the term synonymously with "group" without reference to the subjective and moral elements involved.

[1] From the Institute of Child Welfare, the University of Minnesota. Condensed for periodical publication by Mary Shirley. Two other articles on social behavior of young children by this author will appear later.

The biographical studies included observations of social behavior incidental to psychic behavior. Following the method of Preyer, very comprehensive records of the behavior of little children were made by scientifically-minded parents and others interested in child nature. The works of Major, Moore, Shinn, Fenton, Hall, Tracy, Waddle, Compayre, Hogan, Hug-Hellmuth, Drummond, Rasmussen, Stern, Baldwin, and Cooley are noteworthy. Although the generalizations are usually based upon the behavior observed in one or two children, they present points of view that suggest further research. Stern's book in particular contains an excellent treatment of play behavior. It should be mentioned also that since the investigator is usually a part of the group situation in the home and since there are very few groups of individuals of pre-school age in the homes where these investigations are made, adult social behavior becomes inextricably recorded with child social behavior.

One of the first studies to employ the method of group rather than individual observation on pre-school children was directed by Ruth Andrus (1) in several nursery schools about New York. Sixty-nine student observers each made a diary record while watching one child's activity and later classified the data under the general categories of mental, emotional, motor, and social-moral habits. Despite subjectivity of the ratings and overlapping of the categories this inventory of habits and the rating scale derived from it are improvements over the general impressionistic method of determining a young child's social development.

Gesell (14) listed certain types of personal-social behavior that might be expected at various age levels, after having observed five hundred children, fifty at each of ten age levels.

Ethel Verry's (34) study of mental and social attitudes of children in pre-school play groups indicates the wealth of data that may be obtained by observing the spontaneous groupings of children. She distinguishes five social attitudes: treating playmates as objects, assuming an adult attitude, seeking attention, doing as others do, and coöperating with the group. She found a growth in both the number and complexity of coöperative activities with an increase in age.

Another study of spontaneous grouping is that recorded by Chevaleva Janovskaja (8) in which children in pre-school institutions were observed by teachers over a period of two years.

From diary records, Bott (5) devised a schedule of observation that enabled observers to note the materials used in play, the child's relations with adults, and his relation to other children.

His relations with adults were grouped under the categories of: child asserts; child negates; child resists; adult stimulates; adult restrains; and with children were classified as: talking; interference; watching; imitation; and coöperation.

Bridges (6) in an investigation of the occupational interests of three-year-old children, observed ten children during the free-play hour for several days, recording the time each child took out a set of material and the time when he put it back to change to something else. She found marked uniformities in the toy preferences, both for the group as a whole and for each of the sexes.

Challman[2] investigated "Factors influencing friendship among pre-school children" by recording spontaneous play groups. He developed friendship indices for the children, using sigma deviations and studied the relation of sex, age, mental age, height, extroversion, attractiveness of personality, and sociality to friendship.

Thomas (32) describes three general techniques: (1) those in which each child is followed for a given period in the nursery school and a given overt social-behavior act recorded each time it recurs; (2) those in which, within the larger nursery school situation, a specific social situation is recorded each time it recurs; (3) those in which the psychological test situation, involving more limited social and material stimuli, is used instead of the nursery school for recording data of the sort described under (1) and (2). Because the data do not lend themselves to scientific analysis, the emphasis of these studies is upon method rather than results.

One of the first truly experimental investigations of social behavior was that of Charlotte Bühler (7) made upon 114 nursing children in a New York milk depot. She invited pairs of these children into a room, gave them various toys, and then recorded their reactions. By the end of the first year of life she found children had developed definite personality traits.

Somewhat similar to the study just mentioned, but considerably more objective and controlled, was the investigation carried on by Marjory Walker[3] wherein social reactions of fourteen children in a conflict situation were observed. Children were invited, two at a time, into a test room where one toy had been placed. The shutters in the room enabled the investigator to look into the room and observe the two children without being seen by them. Reac-

[2] Child Development, III, 146–158. 1932.

[3] Walker, Marjory. Social Interaction from the Standpoint of Domination and Subordination. (Unfinished Ph.D. thesis, University of Minnesota.)

tions were recorded in five-second periods on a schedule prepared after much preliminary observation.

Sorokin (27) attempted to measure the differences in efficiency in the performance of set tasks under the incentive of individual and of group reward. Altruism was also studied by noting the diligence with which the children worked when they were to receive the prize for themselves and when the friend of their choice was to receive the reward. It is of value to know that children of pre-school age lend themselves very well to this type of experimentation.

Marston's study of introversion and extroversion (21) in which trait ratings were correlated with the results of experiments designed to measure four traits, namely, social resistance to a stranger, compliance with the request to perform a difficult task, interest reactions to a varied and novel environment, and self-assertion to the denial of a wish, may rightly be considered a study of social interaction. Marston found marked differences in the types of reactions which children made to the various test situations. Of much significance is his finding that introversion tends to increase with age, even at these early age levels. The experiment was conducted in such a way that the investigator was always an element in the situation. Since most life situations are of this type, *i.e.*, an adult is usually present, perhaps the research workers of the future will concern themselves more with the child-adult interaction than with the child-child behavior.

This rather cursory review of some of the investigations [4] which have taken place in the field of genetic sociology and psychology should account for the present state of organized knowledge in these sciences. As yet, the study of child behavior is too new to expect more than scattered individual attempts to find ways and means of penetrating the field which was first revealed to us only fifty years ago.

DESCRIPTION OF SUBJECTS AND OF METHOD OF OBSERVATION

This investigation was carried on in the Nursery School of the Institute of Child Welfare at the University of Minnesota. The applicability of the findings of this study to children as a whole is a function of the similarity of the subjects studied to children in general. In so far as these individuals are representative children, and if the sample is sufficient, the generalizations should hold true. The 42 children observed are summarized as

[4] Further references may be found in the article by Frank K. Shuttleworth (26).

to intelligence, sex, occupational category of the father, age and size of family in Table I.

The intelligence tests which have been given to the children seem to indicate that their average mental ability is above normal, although the I.Q.'s range from 81 to 145. The occupations of the fathers were grouped into categories based upon the Barr Scale for Occupational Intelligence and the Taussig Industrial Classification.[5] Group I is composed of the highest or professional class while the semi-skilled laborers constitute Group V. The occupations are representative of the economic groups of the city of Minneapolis, except for the fact that there is an over-weighting with children from the professional classes. The children were from mixed national stock and came from families where the number of children ranged from one to five.

TABLE I

DISTRIBUTION OF THE CHILDREN OF THIS STUDY IN AGE, SEX, I.Q., PATERNAL OCCUPATION AND FAMILY SIZE

Age at October, 1926	No. Cases Boys	Girls	I.Q.	Cases	Occupational Class	Cases	Family Size	Cases
Under 2	2	2	80–89	2	Group I	13	1 child	5
2–2, 11	9	5	90–99	3	Group II	6	2 children	15
3–3, 11	8	6	100–109	8	Group III	11	3 children	11
4–4, 11	3	7	110–119	11	Group IV	6	4 children	5
			120–129	11	Group V	6	5 children	6
			130–139	3				
			140–149	4				
Total	22	20		42		42		42

Period of Observation

The observations extended from October 26, 1926, to June 10, 1927, with the majority of the observations during the months from January to April. The records from October to January were made when the technique of observing was being developed. During May and June the records were not kept daily because weather conditions permitted the children to play outside and it was thought that elements might enter into outside play which did not exist in indoor play. To complete records, however, some observations on outdoor play were made for subjects who entered the nursery school late in the year.

In order to provide a minimum of variation in the conditions of observation, the investigation was carried on at the same hour every day that the children were in the nursery school. The hour

[5] Descriptions of both of these scales may be found in Terman *et al.*, "Genetic Studies of Genius", Stanford University Press, 1925, pp. 66–72.

selected for observation was a morning period from 9:30 to 10:30 during the free-play period. At this hour every child is permitted to play with any toys he wishes and with any children, or with none at all, as he desires. The teachers make relatively few suggestions to the children, but are in sight of the children in order to help settle any problems that may arise. The sandboxes are opened at the beginning of the hour, the kiddie-kars are placed in the gymnasium upstairs, and practically all toys are accessible to the children without assistance from adults in the room. Since there are not enough toys of every type to supply each child, there is a ruling that the child who gets a toy first, may play with it until he leaves it, or in the case of the swings and large apparatus, the children must take turns. A few activities are directed by adults during this hour, such as painting water-color pictures, washing dolls' clothes, or making valentines; but in no case are the children solicited to join in these activities. If they do join, it is of their own volition.

There are about two assistants or teachers to every room. Occasionally there are four or five observers who sit quietly near the door, and who do not speak to anyone except to nod a reply to the questions from the children. As a rule, the players are quite oblivious to the presence of adults and pursue their activities as if no grown-ups were around.

Categories of Social Participation

Two aspects of social participation may be considered, *extensity,* or the number of social contacts made by an individual, and *intensity,* or the kind of groups participated in and the rôle of the individual in those groups. The number of social contacts may be measured by recording the number of different groups in which a child played. Such a record, however, fails to bring out the differences in social participation between the child who is actively playing in a group and one who is merely an accidental member. Intensity of participation may be determined in two ways: first, by the extent of group integration, *i.e.,* whether the group is organized in such a way that certain duties and responsibilities are demanded of its members, or whether it is only a congregation of independent individuals; and second, by the status of the individual in the group, *i.e.,* whether or not he is helping to shape the plans and activities of the group, *i.e.,* that is to say, his leadership.

After several weeks of preliminary observation of the children

at play the extensity and group integration were combined in such a way that a scale of social participation with rigidly defined categories was worked out. One child, for example, did not seem to be playing at all. He usually stood in the middle of the room, pulling at a handkerchief which was tied to his blouse. His head dropped from one shoulder to the other. If asked what he wanted to do, he would merely shake his head; if a toy were placed in front of him, he would not look at it. This type of behavior was called *unoccupied,* although the child was really playing in the manner designated by Stern as the play limited to the child's own body. In order not to confuse this type of play with solitary play, unoccupied behavior was defined as follows:

> *Unoccupied behavior*—The child apparently is not playing, but occupies himself with watching anything that happens to be of momentary interest. When there is nothing exciting taking place, he plays with his own body, gets on and off chairs, just stands around, follows the teacher, or sits in one spot glancing around the room.

Closely related to the unoccupied behavior is the play in which the child observes a group of children playing, but he himself does not overtly enter into the play activity. He is an *onlooker.* Such behavior was described as follows:

> *Onlooker*—The child spends most of his time watching the other children play. He often talks to the children whom he is observing, asks questions, or gives suggestions, but does not overtly enter into the play himself. This type differs from the unoccupied in that the onlooker is definitely observing particular groups of children rather than anything that happens to be exciting. The child stands or sits within speaking distance of the group so that he can see and hear everything that takes place.

A third type of play behavior is that which is usually called playing alone or *solitary play.* Contrary to general opinion there is no clear-cut distinction between group and solitary play. This is particularly true when the play space available for thirty-six children is too meager to permit them to get out of speaking or hearing distance of one another. For this reason, in borderline cases, a purely arbitrary distinction was used to discriminate between group and non-group play. It was decided that a child who played with toys different from those of the children within speaking distance of himself, and one who centered his interest upon his own play, making no effort to get close to and speak to other children, was playing alone. This play was defined thus:

Solitary independent play—The child plays alone and independently with toys that are different from those used by the children within speaking distance and makes no effort to get close to other children. He pursues his own activity without reference to what others are doing.

Closely related to such individual play is a type of group play which was called:

Parallel activity—The child plays independently, but the activity he chooses naturally brings him among other children. He plays with toys that are like those which the children around him are using, but he plays with the toy as he sees fit, and does not try to influence or modify the activity of the children near him. He plays *beside* rather than *with* the other children. There is no attempt to control the coming or going of children in the group.

A common example of this type of play may be observed in the group who congregate around the sandbox. Several children stand close to one another around the sandbox, each child going after and using the toys with which he wishes to play in the sand (usually cups). Children come and go all the time, but those remaining at the sandbox pay no attention to the movements of others; they are absorbed in their own activities. This type of play is not solitary play, yet it is independent play in a group. To what extent children choose to play with toys because they bring them into physical proximity to other children one can not observe; only the overt facts, not the motives, are observable. Suffice it to say that parallel play better resembles group play, and is a more socialized form of play than solitary independent play.

Two other types of group play were *associative* play and *coöperative* or *organized supplementary play.* Associative play is group play in which there is an overt recognition by the group members of their common activity, interests, and personal associations. Organized supplementary play is the most highly organized group activity in which appears the elements of division of labor, group censorship, centralization of control in the hands of one or two members, and the subordination of individual desire to that of the group. Associative play is a less well organized form of the group activity in which the children play with one another, while organized supplementary play is the type in which the efforts of one child are supplemented by those of another for the attainment of a final goal. They were characterized as follows:

Associative play—The child plays with other children. The conversation concerns the common activity; there is a borrowing and loaning of play material; following one another with trains or wagons; mild attempts to control which children may or may not play in the group. All the members engage in similar if not identical activity; there is no division of labor, and no organization of the activity of several individuals around any material goal or product. The children do not subordinate their individual interests to that of the group; instead each child acts as he wishes. By his conversation with the other children one can tell that his interest is primarily in his associations, not in his activity. Occasionally, two or three children are engaged in no activity of any duration, but are merely doing whatever happens to draw the attention of any of them.

Coöperative or organized supplementary play—The child plays in a group that is organized for the purpose of making some material product, or of striving to attain some competitive goal, or of dramatizing situations of adult and group life, or of playing formal games. There is a marked sense of belonging or of not belonging to the group. The control of the group situation is in the hands of one or two of the members who direct the activity of the others. The goal as well as the method of attaining it necessitates a division of labor, taking of different rôles by the various group members and the organization of activity so that the efforts of one child are supplemented by those of another.

To illustrate the difference between parallel, associative and organized supplementary play, the sandbox situation may be cited.

Sandbox situation

Parallel activity—Several children are engaged in filling cups in the sandbox. Each child has his own cup and fills it without reference to what the other children are doing with their cups. There is very little conversation about what they are making. No one attempts to tell who may or may not come to the sandbox, so children are coming and going all the time. Occasionally one finds a child who remains at the sandbox during the entire period. The children play *beside* rather than *with* one another.

Associative play—The children begin to borrow one another's cups, they explain why they need two cups; they advise and offer sand to one another. They call a child to the sandbox, and ask those present to make room for him. The others may or may not move over, depending upon their own wishes. No child or children dictate what the various children shall make, but each makes whatever he pleases. Someone may suggest that they all make a road but in that case each child makes his own road, or none at all, as he chooses, and the other children do not censor him. There is much conversation about their common activity.

Organized supplementary play—One child suggests that they are all making supper. Soon the various family rôles are assigned or adopted and the children speak about their shares in preparing the meal. Domination by one or more of the children occurs, one child being informed that he can't cook because he's the baby. The group becomes closed to some children and open to others, depending upon the wishes of the leaders. The children are criticized by one another when they do not play their rôles correctly. They are not permitted to leave the sandbox unless it is known what they are going to do next.

One Minute Sampling Observational Method

In order to obtain a measure of the frequency of group participation each nursery school child was observed for one minute daily according to the method of repeated short samples described by Goodenough (15). To prevent spectacular behavior on the part of a given child from being a factor in the observer's transition from child to child the order of observation was determined by a prearranged list of the children's names that was systematically varied from day to day. Further errors of sampling were guarded against by dividing the hour into five-minute intervals and rotating observations so that each child was observed an equal number of times during the first 5 minutes of the hour, the second 5, the third 5, and so on.

Mimeographed forms provided space for recording: (1) the names in the order of observation; (2) the initial letter indicating the degree of participation, u—unoccupied, s—solitary, o—onlooker, p—parallel, a—associative, and c—coöperative; (3) the names of all other children in the group; (4) the number of children in the group; (5) a key letter indicating leadership behavior;[6] (6) a T or a Q to indicate that the child under observation was talking or was quiet; (7) a brief description of the activity in which the child was engaged; (8) his remarks; and (9) duration of the activity estimated in terms of v—short time, m—most of the minute, e—entire minute.

RELIABILITY AND CONSISTENCY OF OBSERVATIONS

The number of minute samples necessary to insure an unvarying picture of social participation was determined by assigning arbitrary weights to each category, summing the scores for the odd and even days for individual children, and running correlations.

[6] The topic of leadership will be dealt with in a later paper.

The weights were as follows:

Unoccupied behavior	(U)	—3
Solitary play	(S)	—2
Onlooker behavior	(O)	—1
Parallel play	(P)	1
Associative play	(A)	2
Coöperative or Organized Supplementary play	(C)	3

For 19 children observed over a 20-day period the composite scores for the 10 odd days correlated with those for the 10 even days by the rank difference method yielded a coefficient of .76. When twenty odd-even day observations were weighted, scored, ranked and correlated the coefficient was .90. Hence twenty one-minute observations give a reliable portrayal of a child's participation in group activities. Since there were 35 children twenty-one observations permitted every child to be observed three times during the seven five-minute sections of the play period. Because of the comparatively high reliability of twenty samples, many of the conclusions drawn from the data obtained in this investigation are based upon this number.

Errors of Observation

As a check upon the objectivity of the descriptive categories three assistants kept records simultaneously with the writer. These observers received no preliminary instruction other than a printed sheet containing the definitions of the symbols used. Each was given a stop-watch which she set in motion upon the signal of the investigator, and all four observed the same child for a minute, and then filled in the schedule in accordance with her understanding of the meaning of the symbols and of the behavior just observed. Since four observers could not view the child from exactly the same position, one would expect to find a few minor differences in the records. Unfamiliarity with the symbols, for none of the observers had studied them for more than a few minutes, caused some erroneous records. Just how much of the variation in records could be attributed to lack of knowledge of instructions and how much to different interpretations cannot be ascertained.

On each of sixteen children thus observed each observer made seven notations (*i.e.*, type of group participation; name of playmates; number of children in group; leadership status; conversation; type of game or toy; comments of child) which amounted to a total of 112 records for each observer. For convenience the items were considered of equal value. The proportion of items identical with those of the writer averaged 89 per cent for the

three observers. One observer agreed in 92 per cent, another in 89 per cent, and the other in 86 per cent of the items. In social participation, using the categories of this study, Goodenough reports a correlation between scores of two independent observers of .76 (Brown-Spearman correction). The combined scores of social participation and leadership reduced the differences between the two observers very greatly—the correlation being .98. It seems reasonable to assume that the bias of the observer or the errors of observation were not a very significant factor in this investigation.

TEACHER'S ESTIMATES

As an additional check on consistency and to determine whether the rough impressions of teachers might be substituted for the detailed method used in this study teacher's estimates were obtained from three teachers and two research assistants who had spent varying amounts of time with the children during the free play period of the year 1926–27. These estimates were made during the first week of school in the fall of 1927. Possibly the interval of three months which had elapsed between June and September might have caused all recollections of play associates except the most outstanding to recede from the memory. For that reason, if this study were made again, these records should be obtained at the end of each month. The teachers stated that they had great difficulty in remembering instances in the detail called for on the estimate sheet. A separate estimate sheet was filled out by each teacher for each child. The sheets provided space indicating the type of social relationship that obtained between the child whose name headed the sheet and every other child in the nursery school. Complete descriptions of the categories of social participation and leadership were attached to the estimate sheets and the teachers were asked to study these before filling in the sheets.

Individual scores on the teacher's estimates were worked out by multiplying the number of times each symbol, representing a type of group, appeared on a child's sheet by the appropriate weight and summing these weighted scores. For example, teacher A recalled having seen child L3 playing in a parallel situation with one child, in an associative situation with four children, and in a coöperative situation with ten different children. L3's social participation rating would be 39. (Parallel—1, Associative—8, and Coöperative—30.) After the teacher's ratings were computed for each child, they were arranged in rank order and correlated

TABLE II

TEACHER'S RATINGS OF SOCIAL PARTICIPATION

Teacher's ratings correlated with composite Social Participation Scores based on 60 one-minute observations on each of 34 children. Scores and ratings* are weighted as follows:

	Weight		Weight
P—Parallel Activity	1	O—Onlooker	—1
A—Associative Activity	2	S—Solitary	—2
C—Coöperative Activity	3	U—Unoccupied	—3

Teacher	Correlation with PAC Scores
A	.65
B	.79
C	.71
D	.59
E	.75

Combined ratings correlated with PAC

A–B	.80
A–B–C	.79
A–B–C–D	.84
A–B–C–D–E	.88
A–B–C–D–E..... Correlated with PAC Minus USO score	.86

Intercorrelations of Ratings

	B	C	D	E
A	.57	.66	.56	.49
B	..	.59	.46	.64
C	..	..	.55	.45
D	..	..	..	.41

* Ratings did not include Onlooker, Solitary and Unoccupied Play so PAC scores also omitted them.

with the scores obtained by sixty one-minute observations of each individual. The results are presented in Table II. Several conclusions may be drawn from these correlations:

The agreement between the Social Participation ratings arrived at from the general impressions of teachers and those obtained through systematic observation is very close. The ratings of one teacher correlated .79 with the scores obtained after sixty one-minute observations, while the most dissimilar rating correlated with the observation .59.

The ratings of several teachers correspond with scores derived from direct observation more closely than do those of any individual teacher.[7] The correlation estimates of five teachers and the scores obtained by the sampling method was .88, a higher figure than is usually obtained from teacher's ratings.[8]

[7] These combined scores were obtained by adding the rank order position of each child and then ranking the total.

[8] Since only the PSC, or Parallel, Associative, and Organized Supplementary scores were used in teacher's ratings, the Social Participation scores with which the ratings were compared were the PSC scores also. When the non-social scores, or Unoccupied, Solitary, and Onlooker (UIO) were subtracted from the PSC scores as was done in Chapter IV, the correlation decreased from .88 to .86.

There is marked uniformity in the scoring among the teachers. The lowest correlation between their scores was .41 while the highest was .66.

The agreement between the ratings derived from teacher's impressions and the scores obtained by the one-minute sampling method corroborates the findings on consistency of social behavior brought out by the even-odd day check.

For most purposes, teacher's ratings on social participation as measured in this study, should prove to be a satisfactory substitute for the one-minute sampling method.

RELATIVE AMOUNT OF SOCIAL PARTICIPATION

The group participation of the children was classified into categories which described lack of group behavior as well as the presence of it. Unoccupied, solitary, and onlooker activity might be considered negative indices of social activity; while parallel, associative and coöperative or organized supplementary play might be regarded as positive indices of social participation.

Since the number of minute observations obtained on each child varied from 12 to 100 the actual number of times each child was observed in each play situation could not be used as an index of the group participation of the child. Instead percentages had to be used. Table III expresses these data for the forty-two children who were observed during the year. The facts of this chart may be interpreted as follows:

There are great individual differences in the proportion of times each of the forty-two children were observed in the various types of play situations.

Unoccupied behavior—Unoccupied behavior was observed in only five children. Three of these children were unoccupied quite frequently (11 and 12 per cent of the time) while the other three were so observed 2 and 3 per cent of the time.

Concerning the three who were unoccupied more or less habitually:

All three were under three and one-half years of age. They were the least talkative children in the school (their rankings were 1, 3, and 4).

Two were girls and one was a boy.

All had older brothers and sisters, and one had one younger brother.

They had had as much nursery school experience as most of the other children (their rankings were 5, 14.5, and 26).

TABLE III

SOCIAL PARTICIPATION

Showing the per cent of each child's observations that were spent in each type of social participation.

Child	Total Obs.	Per cent U	Per cent S	Per cent O	Per cent P	Per cent A	Per cent C
J4	33	..	6.2	15	27	27	25
I4	43	..	15	5	35	35	11
H4	41	..	17	20	46	15	2.4
G4	42	..	7.6	24	12	33	24
F4	40	..	10	5	30	30	25
E4	98	..	17	..	33	33	17
D4	97	..	7	4.1	20	48	21
C4	96	..	7	2.3	35	35	20
B4	78	..	5.2	9	32	34	20
A4	29	..	3.4	3.4	31	31	31
N3	79	..	1.2	1.2	8	33	57
M3	100	..	18	5	51	18	8
L3	76	..	12	11	22	33	22
K3	76	..	25	12	25	33	5.2
J3	77	..	15	9	33	21	22
I3	82	..	6.2	3.4	15	37	39
H3	70	..	11	4.1	25	30	30
G3	75	..	22	5.2	33	23	17
F3	71	..	4.1	4.1	26	45	20
E3	89	..	33	7	43	15	2.4
D3	82	..	20	7	33	20	20
C3	80	..	3.4	6.2	34	21	36
B3	89	..	8	2	32	28	30
A3	92	..	10	2.4	33	35	20
N2	87	..	30	3.4	32	20	15
M2	78	..	17	12	9.9	37	25
L2	85	..	17	11	54	11	8
K2	90	11	23	38	20	7	1
J2	73	..	20	5.5	50	20	5.5
I2	78	12	33	10	44	..	1.4
H2	73	..	33	12	34	20	1.3
G2	88	3.4	26	11	39	17	4.1
F2	74	..	12	15	33	33	6
E2	64	..	25	11	40	12	12
D2	64	..	25	4.6	50	20	1
C2	86	3	35	8	20	26	7
B2	77	..	21	1.2	42	40	6.2
A2	19	..	5.2	20	35	35	5.2
D1	75	12	9	6	62	8	2
C1	77	..	25	2.5	48	22	3
B1	68	2.9	30	10	50	7	1.4
A1	12	..	17	..	58	17	8

Their I.Q. rankings were 31, 28, and 14, which is above average for two of them, and slightly below for the older child.

Solitary play—Solitary play was common to all the children but with much variation. One child was observed to be playing alone only 1.2 per cent while at the other extreme, four children played alone 33 per cent of the time under observation.

The child who seldom played alone was an only child of young parents. He was older than thirty of the children and had been in the nursery school longer than twenty-six of them. His I.Q. was about average (111). He was an outstanding leader of the school, and played in highly organized group situations about 90 per cent of the time.

The four children who played alone one-third of the time ranked 21, 10, 11, and 5 with respect to age. Although they were younger than the average, they were by no means the youngest children in the school. They came from families in which there were 3, 4, 2, and 1 older siblings. They had all been in the nursery school longer than the average of the children (their nursery school experience rankings were 30, 17.5, 26, and 21.5). So their solitary play could not be explained by lack of acquaintance with the other children. Their I.Q.'s were near and below average—their rankings being 19.5; 6; 10; and 17.

The mothers of two of the four children were interviewed and a correspondence between school and home behavior was apparent. One mother characterized her son as a shy boy who did not play with other children very much in spite of the fact that there were four older children in the family. He was perfectly content to "go off on his own track." He never mentioned any of the nursery school children or teachers at home. The other mother stated that her little girl was very independent; often would refuse to give in to the wishes of her older sister; and, as a result, would have to play alone. She never talked about any of the children at the nursery school.

Onlooker behavior—Although all but two children were found in onlooker situations, this type of activity was not as frequently engaged in as were solitary and coöperative play. One child, however, was observed to be an onlooker 35 per cent of the time. He was also unoccupied a large part of the time. He took a longer time than any other child to adjust himself to group situations. He entered the school October 5, 1926, and was not observed playing with another child until January, 1927. The cause of his reticence could not be traced to any known factor. One teacher hinted that it might have been due to the fact that the child was large for his age, and had been placed with the group of older children during music and conversation periods. He must have felt their physical and mental superiority, and therefore lacked the courage to assert himself. At home, he had two older brothers and two older sisters who "took care of him."

There must have been several elements in common between unoccupied behavior and onlooking activity as the children who ranked high in one type usually ranked high in the other.

Parallel activity—Parallel activity was engaged in frequently by almost all the subjects. Eight children were so occupied over half the time they were observed. In general, the younger children par-

ticipated in parallel groups more than did the older ones. This is partly explained by the type of toys with which the children played, *i.e.*, the sandbox; it facilitates and usually accompanies parallel play and is quite a favorite among the younger children as it does not call for any great physical or mental skill.

The child who ranked consistently low in non-group participation and very high in organized groups played in parallel groups less often than any other child or children. Only 8 per cent of his recorded play was in parallel groups. Two-thirds of the children, on the other hand, played in such groups over 33 per cent of the time observed.

There seemed to be no consistent sex or I.Q. differences between the children who did or did not participate in parallel groupings. The nursery school experience for six of the eight children who were occupied in parallel play over 50 per cent of the time, was less than that of the average child. Since length of the period of attendance at the school is largely determined by age, it seems reasonable to attribute parallel play to the age factor rather than to familiarity with the nursery school situation.

Associative play—All the children except one were observed playing in associative groups. Sixteen individuals were so observed over one-third of the total number of observations, and one child played in these groups one-half the time.

Twelve of the sixteen children were over three years old. It may be concluded, therefore, that overt interest in playmates and their activities is largely confined to the older pre-school children.

Organized supplementary or coöperative activity—The proportion of observations that children were engaged in coöperative play varied from 1 to 57 per cent. Of the six individuals who played in organized supplementary groups during 30 or more per cent of the time observed, all were over three years old. Two were girls and the other four were boys. Their mean I.Q. was 120. Of the five individuals who were observed in coöperative situations less than 2 per cent of the time, all were under two years and eleven months. Their mean I.Q. was 113 as compared to the 120 mean of the most coöperative children.

It is evident from the above data that participation in the most social types of groups occurs most frequently among the older children. Furthermore, those individuals who engage in organized supplementary play the least, have a lower I.Q. on the average than have the most coöperative ones.

Average Amount of Social Participation

Another method employed to make the scores comparable for the various individuals was to select sixty observations on the 34 children for whom that number was available. Of those having more than sixty total observations, the first twenty, the middle,

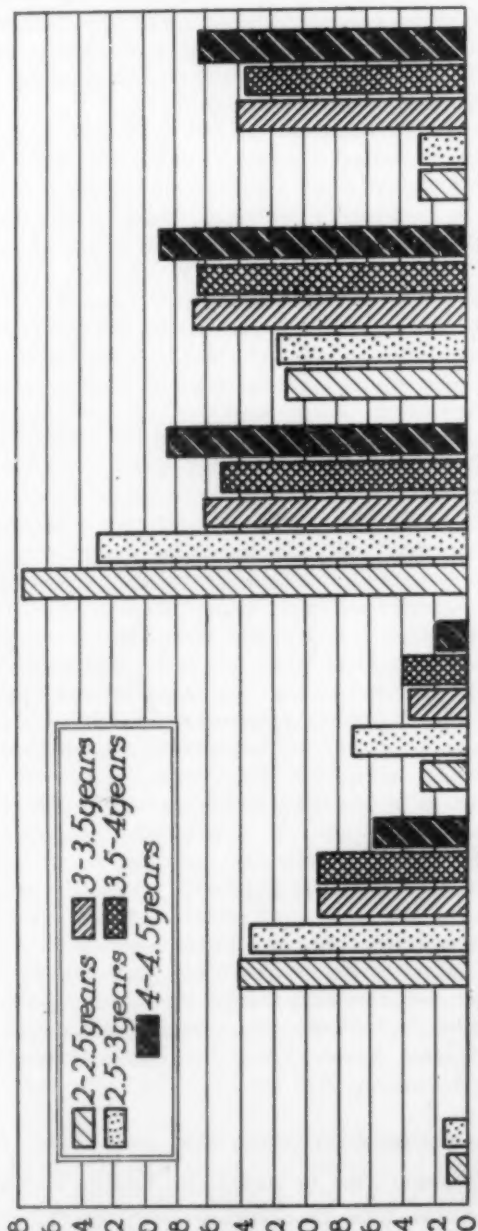

FIG. 1. Social Participation and Age

Showing mean number of times each activity was engaged in at different ages
(Six children at each age level, sixty observations on each child)

and the last twenty observations on each child were the ones selected. Averages for each social participation category were computed for the total number of times each child was found engaged in a particular activity out of a possible sixty times that he might have been so occupied had he done nothing else. For example, the child with a score of 40 for parallel activity, had a score of 3 for onlooker; 6 for unoccupied; 5 for solitary; 5 for associative, and 1 for organized supplementary play, making a total of sixty observations. At the other extreme, there is the record of a child with a score of 4 for parallel; 1 for onlooker; 1 for solitary; 19 for associative, and 35 for coöperative or organized supplementary activity. The following table presents summary of these data for all of the thirty-four children.

Parallel play, it will be seen, is the most frequent type of play and in it the children show a variability equalled only by that in coöperative or organized supplementary play. Onlooker behavior is not very common and in no child does it make up more than one-sixth of the observations. One child never exhibited either associative or organized supplementary play, whereas another was engaged in these more social pursuits 90 per cent of the time. On the average the three unsocial play types, unoccupied, solitary, and onlooker, make up about 25 per cent of the observations, whereas the social types, parallel, associative, and coöperative or organized supplementary make up 75 per cent. Standard deviations are relatively high for onlooker and coöperative or organized supplementary play, and are lowest for parallel and associative play.

TABLE IV

AVERAGES AND VARIABILITY OF 34 CHILDREN IN SOCIAL PARTICIPATION CATEGORIES BASED ON 60 OBSERVATIONS EACH

	Range	Mean	S.D.
O—Onlooker	1–10	4.5	3.6
S—Solitary	1–24	10.5	6.
P—Parallel	4–40	19.	7.8
A—Associative	0–27	14.	6.6
C—Coöperative or Organized Supplementary	0–35	9.5	7.8
AC	0–54	25.	12.9
PAC	15–58	44.	9.2
USO	2–45	15.5	9.

In order to determine the extent to which the various types of play activities were measuring like phenomena they were correlated with one another. Since the categories into which social participation was classified are mutually exclusive, a high score in one activity would necessarily result in a lessened score in the other activities. Nevertheless, a comparison of the activities, two

at a time, brings out significant relationships. The following data present the results of these correlations:

TABLE V

CORRELATION OF ACTIVITIES WITH ONE ANOTHER

(Spearman rank order)

S correlated with C	—.66
S correlated with A	—.55
S correlated with P	.36
S correlated with AC	—.69
C correlated with A	.44
AP correlated with C	—.14
P correlated with A	—.60

Organized supplementary play is positively correlated with associative play to such an extent (.44) that is likely that they have many identical elements. When associative play is combined with parallel activity and correlated with organized supplementary play the relationship is practically eliminated. From this it may be inferred that those individuals who engage in parallel activity are not usually the same ones who also engage in more socialized activities. The large inverse correlation between parallel and associative play (—.60) indicates that the children who play in associative situations do not, as a rule, tend to engage in parallel play. Solitary play is correlated inversely with associative and coöperative play (—.69), but is positively correlated with parallel play (.36). This means that those children who play alone are occasionally the same ones who engage in the most elementary type of group play, and who seldom play in the more highly organized types.

Weighted Scores on Social Participation

The weighted scores on the 60 observations for each of the 34 children were obtained by multiplying the frequency of each category by the assigned weight and getting the algebraic sum. The scores were distributed as follows:

Scores	No. Children
—60–0	2
1–9	4
10–19	2
20–29	2
30–39	4
40–49	4
50–59	2
60–69	2
70–79	1
80–89	4
90–99	5
100–149	2

They ranged from minus 54 to plus 144. The individual receiving the lowest score was described above as an habitual on-

looker. His I.Q. was 121. Since he came from a home where there were five children, he should have had an opportunity to learn how to play with other children. Possibly playing with members of one's family does not require the social adjustments as does playing with strange children.

The child who received the highest score was an only child. His I.Q. (111) was close to the average and he ranked tenth from oldest child in the nursery school; he was also an outstanding "leader". Being a leader would, of course, necessitate his playing in coöperative situations. Aside from this fact, no known factors account for his excessive group participation.

AGE AND SOCIAL PARTICIPATION

The rank order scores of the children in each type of play activity were correlated with their ages. The results are shown in the following data:

TABLE VI

SPEARMAN RANK ORDER CORRELATION BETWEEN AGE AND ACTIVITIES

Age correlated with S	—.58
Age correlated with P	—.71
Age correlated with USO	—.71
Age correlated with A	.51
Age correlated with C	.67
Age correlated with AC	.69
Age correlated with PAC	.56

These correlations clearly indicate the important influence of age on social participation. The older the child, the more he plays in the more highly integrated groups. All the correlations between age and the unsocial play types are high and negative, and all those between age and socially organized play are high and positive.

Another technique employed to determine the influence of age upon social participation was the grouping of children by half year intervals from two to four and one-half years, making a total of five groups. This classification was somewhat crude and only six children represented each age. Nevertheless striking age differences were found as may be noted from Figure 1, which represents the mean number of times (taken from a total of sixty observations for each child) in which the six children at each age participated in social groups.

Only the youngest children, those from two to three years, were found unoccupied during the sixty observations. Solitary play was most common at two and one-half years but there is a decided decline

in the importance of solitary play at three and again at four years. Onlookers were most prevalent among the two-and-one-half to three-year-olds. Evidently, children at this age have overcome the shyness existing when first placed among strange children. They are beginning to take an interest in group activity although they do not overtly participate. The oldest children do not engage in onlooking frequently. If an activity interests them they want to participate in it. They know all the techniques of securing entrance into a group, so do not remain observers for any noticeable length of time. Parallel play groups were observed most often among the two-year-olds, and least often among the children from three to four. As children became older, they invariably conversed with one another about their activities, and became interested in their associates. Associative group play increased in popularity as the children became older, and was most frequent in the oldest group. There is a marked increase in organized supplementary play beginning with the third year. This sudden interest in coöperative play is perhaps accounted for by the popularity of the activity of "play house" among the three-year-olds. Since the young children lack the power of expressing themselves with language, they have difficulty in playing in coöperative groups.

Another check upon the relationship between age and social participation was made by correlating the age ranks with the ranks of the composite scores, which yielded a coefficient of .61. Thus, a definite relationship between the age of children and the degree to which they participated in social groups was established.

INTELLIGENCE QUOTIENTS AND SOCIAL PARTICIPATION

The intelligence quotient ranks were correlated with the ranks of forty-two children in each of the types of participation mentioned below. The results obtained were:

I.Q. correlated with P	.69
I.Q. correlated with S	—.20
I.Q. correlated with C	.38
I.Q. correlated with A+C	.33

The composite social participation score of thirty-four children was also correlated with their intelligence quotients with a resulting coefficient of .26.[9]

Some relationship between social participation and intelligence quotients is evident in the above coefficients. Parallel play particularly is associated with intelligence (.69). Since parallel play is most popular among the two-year-olds, it is conceivable that those young children who possess the most intelligence manage

[9] Lehman and Anderson in an article, "Social Participation vs. Solitariness in Play" (Pedagogical Seminary, Vol. 34, 1927, pp. 279–89) found that older children with an I.Q. over 140 engage less frequently than the average children in social plays and games.

to play in groups, even though very elementarily, rather than play alone. On the other hand, those two-year-old children who are least impressed by the new social environment of the nursery school into which they are placed will be content with solitary play. In other words, those characteristics in children which would cause them to be interested in and curious about various elements in their home environment, resulting in high intelligence test scores, would also show themselves in an interest in one of the leading features of the school environment, the social groups. The fact that solitary play, which occurs most frequently among the younger children, is inversely correlated with I.Q. ranks, seems to substantiate this interpretation. Those types of participation in which the older children primarily engage correlate with I.Q. to a lesser extent. Group play among the children over three years of age is not very indicative of intelligence.

NURSERY SCHOOL EXPERIENCE AND SOCIAL PARTICIPATION

Familiarity with the nursery school environment would seem, a priori, to determine the readiness with which the children entered into group play. However, this was not found to be the case. Children were ranked according to their date of entrance in the school and these ranks were correlated with their social participation rank scores. A coefficient of .12 resulted. In other words, the relationship was insignificant.

CHANGES IN SOCIAL PARTICIPATION OR "SOCIALIZATION"

Although length of attendance at the nursery school did not determine the degree to which any specific child played in groups as compared to other children, continued attendance seemed to bring about changes in the degree to which children as a whole participated in groups. Table VII summarizes these changes for

TABLE VII

CHANGES IN SOCIAL PARTICIPATION

Mean frequency of each type of social participation during the first, middle, and last twenty observations on thirty-four children

	First 20		Middle 20		Last 20	
Unoccupied	.2	σ	.2	σ	.2	σ
Solitary play	3.6		3.9		2.9	
Onlooker activity	2.1		1.1		1.2	
Parallel play	8.3		6.4		5	
Associative play	3.2		4.9		7.1	
Coöperative play	2.5		3.3		3.6	
USO	6.0	3.7	5.5	3.2	4.5	3.2
PAC	14.3	3.7	14.6	3.7	15.4	3.4

thirty-four children. Since twenty observations were proved to be fairly reliable, it was possible to note the changes which took place in the participation of the children over a period of sixty observations.

Those types of social participation to which have been assigned negative values namely, unoccupied, solitary and onlooker activity were less prevalent toward the end of the period of observation than they were at the beginning. The shift away from solitary play, however, was preceded by a period of popularity for this type of play. The most social groups, associative and coöperative, tended to occur more frequently with greater Nursery School experience. Associative play more than doubled in popularity while coöperative play increased from a mean frequency of 2.5 to 3.6. Children played beside one another (parallel play) less frequently as time elapsed. This may be attributed to the fact that parallel play is inversely correlated with age. It may also mean that when children become acquainted, they create interacting situations.

Although the differences between the USO and PAC means are not statistically significant the trend of the changes is consistently toward the more social types of participation. Also, due to the inclusion of parallel play, the differences are not as marked as they would have been had only Associative and Coöperative play been grouped together. These figures show the leveling effect upon individual variation of the nursery school environment.

Changes in participation brought out interesting behavior patterns among the children. Some children varied only slightly around their initial group participation; others initially showed extremes of participation or lack of it, but finally adjusted to an intermediate position; still others swing from one extreme to the other.

SUMMARY

The Social Participation of forty nursery school children was analyzed in this study. The spontaneous play groups occurring during the free-play hour were observed by a one-minute sampling method. Each child was observed during one minute of the hour and his social behavior was recorded on a schedule. Provision was made for rotating daily the order in which each child was observed. Social participation was classified and recorded under categories of Unoccupied; Solitary play; Onlooker; Parallel group activity; Associative group play, and Organized Supplementary,

or Coöperative group play. Leadership, as a phase of intensity of social participation, was recorded as Independent pursuing of own will; Directing; Following; Reciprocally directing, and Intermediate position of following one child and directing another in the same group.

In addition to classifying the social behavior of each child as he was observed, notation was also made of the names and number of children in the group in which the child under consideration was playing, a description of his occupation, and any remarks which he made.

RELIABILITY OF THE SAMPLING METHOD

The reliability of the one-minute samples was tested by the even-odd day correlation. Ten even day observations correlated with 10 odd day observations of social participation .76. With 20 even and odd day samples the correlation coefficient obtained was .90.

Leadership observations were not as reliable as were those on Social Participation. Ten even and ten odd day scores correlated .39; twenty scores of alternate days correlated .44; while thirty sample of even and odd days yielded a correlation coefficient of .73.

A check was made upon the errors which might result from the bias of the observer. Three assistants made observations simultaneously with the investigator while observing sixteen individual children. The records were identical with those of the investigator in 89 per cent of the items noted.

Five teachers were asked to recall the specific playmate combinations which had occurred during the preceding year. These recollections were transformed into ratings which were correlated with the scores obtained by the one-minute sampling method. The five combined teachers' ratings correlated .88 with the Social Participation scores, and .81 with the Leadership scores derived from one-minute samples.

SOCIAL PARTICIPATION

Individual differences in the proportion of time children were observed engaged in each type of activity were analyzed. Unoccupied behavior was observed in only five children, while all children engaged in Solitary play. All but two children were observed in Onlooker situations, and all but one in Associative play groups. Parallel activity occurred more often than any other; two-thirds of the children played in this manner over 33

per cent of the time observed. Coöperative play occupied from 1 to 57 per cent of the observations of the various children.

Each of the categories into which social participation was classified were assigned weights based on the degree to which they were thought to approach coöperative group behavior. Sixty one-minute observations on each child were weighted and the algebraic sum of the added scores was designated as the "Social Participation Score". These scores ranged from minus 54 to plus 144.

Correlation of Social Participation was made with several other factors, as follows:

1. Social Participation is dependent, to a large extent, upon the age of the children. As a rule, the youngest children either play alone or in parallel groups, while the oldest individuals play in the more highly organized groups. Marked individual variation, however, was observed. The correlation between age ranks and the Social Participation ranks was .61.
2. There is a slight relationship between Intelligence Quotients and the degree of Social Participation of individuals (.26). Parallel play, however, correlated .69 with intelligence.
3. Nursery school experience correlated with the Social Participation scores .12.

Changes in frequency of occurrence of the various types of social activity were analyzed. Positive social participation increased over three twenty-observation periods while negative participation decreased for the group as a whole.

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TRAITS ASSOCIATED WITH REGISTRATION AND VOTING [1]

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INTRODUCTION

WHEN a person is legally qualified to register and to vote, the study of the factors affecting his use of this privilege becomes a psychological problem. One of the first attacks upon this problem would seem to be a determination of the habits of registration and voting among different classes of people. Studies of this kind have been made by Merriam and Gosnell (5) and by Arneson (1). A previous study of ours (4) showed that the speeder and the reckless driver register and vote less often than the more law-abiding automobile driver. The present study continues the same kind of analysis.

The study is entirely objective, depending solely upon the use of public and semi-public records.

SUBJECTS

The subjects of this study were residents of Lansing, Michigan, during 1928. The first step in the selection of the subjects consisted in taking the first two names at the top of each page of the 1928 city directory. As stated elsewhere (3), such a selection may not be a random selection of the city's population, but it does constitute a defined and reproducible group. As a second step, those names which did not appear in the 1929 directory were discarded in order to retain only those persons who remained in Lansing long enough to meet the residence requirements for registration and voting (canvassing for the directories occurred in March of each year). While some persons may have moved from and returned to Lansing during that time, they must have been few. As a third step, all names appearing in the 1928 school census were discarded in order to throw out those too young to vote. The final group consisted of 416 men and 422 women.

[1] We are under great obligation to those persons of Lansing, Michigan, who made available the data which we present in this article: They are: Miss Bertha Ray, City Clerk; Mrs. Lydia Pierce, School Attendance Officer; Mr. W. L. Kirby, Manager of Lansing Credit Exchange; and Mrs. F. M. McBroom, Director of the Community Welfare Fund. Mrs. Dennis gave invaluable assistance to all aspects of this research.

The selection of the subjects was such as to insure that practically all of them were legally qualified to vote. The residence requirement in Michigan is 6 months' residence prior to election day. Since all subjects lived in Lansing during March, all would have been there 6 months by the time of the primary election in September. Ward requirements did not prevent voting, but merely designated the ward in which voting had to occur.

Those absent from the city were entitled to vote by mail.

In regard to age, the school census attempted to include all persons under 20 years of age on May 31, 1928. This means that in September our youngest subjects were only 20 years 4 months and in November 20 years 6 months of age, and hence unqualified to vote. However, such cases were too few to affect the results in any material way. The number of persons in our group after step 2 between the ages of 16 years, the directory limit, and 20 years, was 29. Assuming equal numbers of 16, 17, 18, 19 and 20 year old persons, the most probable number of ineligibles included in our final group would be 5 for the primary election and 3 for the general election.

Unfortunately the number of persons in our final group who were unqualified to vote on account of being unnaturalized foreign born was probably greater. In 1920 about 7% of persons 21 years of age and over, living in Lansing belonged to this group. Probably these would be incompletely represented in the city directory, yet these ineligibles must be kept in mind. An undetermined proportion of these had lived in the United States long enough to have gained voting privileges had they so desired. We shall present data of one kind which are entirely free from the influence of ineligibility. These consist of differences in voting among those already registered: all those registered were legally qualified to vote.

SOURCES OF DATA

Those of our subjects who registered and voted in 1928 were determined by checking our card index against the records of the City Clerk. By state law all persons were required to register after June 1, 1928, in order to vote in the primary and general elections of 1928. This required a re-registration of old voters as well as registration of the new.

The presence or absence of the following traits was determined in the case of each subject by the methods indicated:

1. *Married.* By this is meant that a husband and wife lived at

a common address according to the 1928 directory. Persons married but separated were therefore listed as single.

2. *Business or Professional Occupational Status.* This was secured from the 1928 directory. Company executives and officials as well as proprietors were listed as business men.

3. *Lansing Resident for Five Years.* By this is meant that the name of the subject appeared in the 1923 directory.

4. *Same Occupation Five Years.* This signified that the person was listed as having the same occupation in the 1923 directory as in the 1928 directory. Among unskilled workers, this meant working for the same company; among skilled, it meant having the same trade.

5. *Same Residence Five Years.* Living on the same street and at the same number in 1923 as in 1928.

6. *Telephone.* Each name was checked against a telephone directory which was compiled in August, 1928. Persons were credited with a telephone when a person of the same surname and of the same street address appeared in the telephone directory.

7. *Credit Record.* Our file was checked against the file of the Lansing Credit Exchange, to which all of the leading credit stores belong, to determine whether credit had ever been extended.

8. *Perfect Record.* This means that all of the individual's records in the credit exchange files were favorable.

9. *Community Chest Contributor.* This meant that the subject singly, or jointly with husband or wife, contributed some sum to the Community Chest in 1928.

THE DISTRIBUTION OF POLITICAL BEHAVIOR

There were four possibilities of political behavior. First, a person might neither register nor vote. Second, he might register but not vote. Third, he might register and vote in only one election. Fourth, he might register and vote in both elections. The percentage of subjects falling in these classes were respectively, 52, 5, 19, and 24 for the men and 66, 2, 17, and 15 for the women. It will be seen that the first and last two categories contain the largest percentages; in other words, registration really separates two classes of people. Those who did not register could not vote; those who registered nearly always voted in at least one of the elections.

SEX DIFFERENCES IN POLITICAL BEHAVIOR

The sex differences found in this study are disclosed by Table 1. The women were less active in each respect and these differences

between the men and women are highly reliable. It will be seen, however, that the differences in voting were caused entirely by differences in registration. Of those subjects who registered 52.4% of the men and 47.8% of the women voted in the primary and 88.1% of the men and 89.4% of the women voted in the general election. These figures show that the differences are negligible, and that as far as this population and this election is concerned more women than men belonged to the class of unregistered, but that the registered members of each sex behaved in the same manner.

TABLE 1

FREQUENCY OF POLITICAL BEHAVIOR AMONG MEN AND WOMEN

	Per cent Registered	Per cent Voted in September	Per cent Voted in November
Women	33.8	16.2	30.2
Men	48.2	25.2	43.3

MARITAL RESEMBLANCES IN POLITICAL BEHAVIOR

Among our subjects were 247 pairs of husbands and wives, so that an opportunity was offered for the study of marital resemblances. Of the husbands, 53.9% registered; of the wives, 35.2% registered. If the responses of husbands and wives were in no way related, we should expect both members of a couple to register in a proportion of the cases represented by 53.9 times 35.2 or 18.9% of the cases. Actually, however, in 31.6% of the cases both members of the married pairs registered, showing a much greater agreement in behavior than one would expect by chance. Likewise, while upon a chance basis both husband and wife would vote in the primary election in only 4.7% of the cases, in fact both voted in this election in 14.3% of the cases. The chances that both husband and wife would vote in the general election were 14.7 in 100, but 21.7 in 100 actually so voted.

The frequency of behavior by husbands and wives jointly will be found to be nearly as high as that of the wives taken singly. Of the wives who registered, of those who voted in September, and of those who voted in November, 91%, 82% and 90% respectively of their husbands performed the same response. However, of the husbands who registered, voted in September and voted in November, only 58%, 50% and 60% respectively of their wives acted jointly with their husbands. In other words, in families in which the wife voted, the husband almost always voted, but the reverse of this was not true.

THE ASSOCIATION OF TRAITS WITH THE POLITICAL BEHAVIOR OF WOMEN

Table 2 shows the frequency of registration and voting in the group of women as a whole and in five groups segregated on the basis of social traits. The business and professional women were not considered separately because of small numbers. Those

TABLE 2

POLITICAL BEHAVIOR AMONG SPECIAL GROUPS OF WOMEN

	Number	Per cent Registered	Per cent Voting in September	Per cent Voting in November
Entire group	422	33.8	16.2	30.2
Married women	310	34.5	16.8	31.0
Possessors of telephone	156	39.2	25.0	34.7
Those with credit record	242	34.2	17.7	30.6
Perfect credit record	197	33.5	16.8	30.0
Subscriber to community chest	79	43.0	27.8	36.8

groups which were formed by referring to the 1923 directory were not studied in the case of the women because women could be identified only if they were continuously single or continuously married for five years—that is, marriage or divorce changed the name. The five special groups which were studied were only very slightly different from the women as a whole. The most reliable differences from the entire group were shown by the subscribers to the community chest, and the critical ratios of these are only 1.5, 2.0, and 1.1 respectively for the three kinds of political behavior. On the whole, the study has not found any traits associated with political behavior in women, although we shall later discover such traits among men. However, the traits most closely related to political behavior in men were the very four which could not be studied in the case of women.

THE ASSOCIATION OF TRAITS WITH THE POLITICAL BEHAVIOR OF MEN

Table 3 gives the comparable figures for the male group of subjects. It will be noticed that in every case the special groups exhibit more political behavior than the group as a whole. Table 4 gives the critical ratios $\left(\frac{D}{\text{sigma } D}\right)$ of the differences between the special groups and the entire group. Inspection of the table shows that 9 of the 27 differences would arise by chance less than 3 times in 1000, and that all of these reliable differences appear in respect of the four traits Business or Profession, Lansing Five Years, Occupation Five Years and Residence Five Years. The other differences are less reliable.

Does the frequency of registration and voting increase regularly with an increase in the number of these traits possessed by the individual? The answer to this question is given by Table 5. It will be noticed that the answer is in the affirmative. In each column the frequency of response regularly increases until the last group was reached.

TABLE 3

POLITICAL BEHAVIOR AMONG SPECIAL GROUPS OF MEN

	Numbers	Per cent Registered	Per cent Voting in September	Per cent Voting in November
Entire group	416	48.2	25.2	43.3
Married	312	54.2	29.1	48.1
Business or profession	77	67.5	44.2	61.0
Lansing five years	247	59.1	33.6	52.0
Occupation five years	135	63.7	34.8	58.5
Residence five years	131	64.9	41.2	59.5
Telephone	156	54.6	34.6	50.0
Credit record	304	53.7	29.2	48.7
Perfect credit record	212	55.7	30.2	51.5
Community chest	84	58.4	40.5	52.4

TABLE 4

CRITICAL RATIOS OF DIFFERENCES BETWEEN PERCENTAGES OF THE SPECIAL GROUPS AND THE ENTIRE GROUP OF MEN

	Registration	September Voting	November Voting
Married	1.7	1.2	1.3
Business or Profession	3.3	3.1	2.9
Lansing five years	2.8	2.2	2.2
Occupation five years	3.2	2.1	3.1
Residence five years	3.5	3.3	3.3
Telephone	1.4	2.1	1.4
Credit record	1.4	1.2	1.4
Perfect credit record	1.7	1.3	1.7
Community chest	1.7	2.5	1.5

TABLE 5

FREQUENCY OF POLITICAL BEHAVIOR AMONG MEN POSSESSING VARIOUS NUMBERS OF SOCIAL TRAITS

Number of Traits	Number of Subjects	Per cent Registered	Per cent Voting in September	Per cent Voting in November
0–1	65	26.2	4.6	18.4
2–3	112	35.4	20.2	30.0
4–5	110	56.4	23.8	50.0
6–7	80	70.0	45.0	65.0
8–9	33	60.5	48.5	57.6

Earlier in this paper we indicated that we tried to eliminate legal disqualifications, and if we succeeded the preceding tables have shown a psychological difference between the possessors and the non-possessors of certain traits. It is possible, however, that the number of subjects not entitled to vote was greater than we

estimated. In order to show beyond doubt that psychological effects upon voting accompany the possession of the traits here studied, we need only to deal with the behavior of those who registered and whose right to vote therefore cannot be questioned. Table 6 shows the results of this analysis. This shows unmis-

TABLE 6

FREQUENCY OF VOTING AMONG REGISTERED MEN POSSESSING VARIOUS NUMBERS OF TRAITS

Number of Traits	Number of Subjects	Per Cent Voting in September	Per Cent Voting in November
0–1	17	17.7	70.5
2–3	40	57.5	85.0
4–5	62	40.3	88.7
6–7	56	64.5	93.0
8–9	20	80.0	95.0

takably that the possession of a large number of the traits studied is accompanied by greater frequency of voting even among those who are registered. While the numbers are too small to make all group differences reliable, the differences between the lower and the upper halves of the subjects is reliable in each case.

ASSOCIATION BETWEEN KINDS OF POLITICAL BEHAVIOR

We have seen that in the case of women we failed to discover, while in the case of men we did discover traits related to the responses of registration and voting. We need to add that in the case of both women and men various kinds of political behavior give a greater probability of other political behavior than do any of the non-political traits which we have studied.

Among the women who registered, 48 per cent voted in the primary and 89 per cent voted in the general election. These percentages are double those found in any of the special groups of women. Of the men who registered, 53 per cent voted in the primary and 88 per cent voted in the general election. These percentages are higher than those of any special group of subjects; they are even higher than those shown by the subjects who possessed 6–9 of the traits studied. Registration, therefore, gives greater assurance of voting than do any or all of the other facts which we possess concerning our subjects. It should be remembered, however, that Table 6 has shown that the possession of social traits in addition to registration increased the voting frequencies above those for mere registration.

But even the combination of registration and social traits gave no greater assurance of voting in the general election than did the mere fact of having voted in the primary election, for the greatest prediction reached by the former method, 95 per cent,

was exactly the same as the frequency of November voting among those who voted in September (106 cases). These figures are for the men only, as traits were not summed in the case of the women.

DISCUSSION

We realize the hazards that would surround the belief that the results secured from the study of a single election in a single community are capable of any general application, and consequently we shall be content with having demonstrated an objective technique for the study of political behavior which does not require the coöperation or knowledge of the subjects. We believe this technique if widely used would yield valuable results. Within the limits of the present study, the propertied and stable citizens were most interested in voting. Was this caused by the motive of protecting privileges or by something more altruistic or more automatic, such as "belonging" in the community? How do different election issues affect various classes of citizens? Is the acquisition or the retention of benefits the greater motive to voting? Is it not possible that by widespread study of traits associated with registration and voting the complicated motives of political behavior could be to some extent analyzed and weighed? Studies of the voting rates of different classes of citizens yield data on many problems which cannot be attacked by studies of voting rates in geographical areas such as those of Rice (6).

SUMMARY

Subjects were secured nearly all of whom were legally qualified to vote and the social traits and political behavior of the subjects were studied by an objective method not requiring the knowledge or coöperation of the subjects.

The distribution of the political behavior was found to be decidedly U-shaped. Most of those who went to the trouble of registering, voted in one or both elections.

Men of the business and professional classes and men who showed stability of residence and occupation registered and voted reliably more often than the average man. Other traits showed less certain association with the political behavior of men, and the traits studied showed no reliable correlation with registration and voting among the women of our group.

With men the amount of political behavior increased with an increase in the total number of social traits possessed.

Registration was more closely related to voting, and primary

election voting more closely related to general election voting than were any of the non-political traits which were studied.

This study demonstrates a possible method for the investigation of the psychology of political behavior.

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THE ROLE OF DISCUSSION IN CHANGING OPINION REGARDING A MATTER OF FACT * [1]

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IN a previous article[2] the writer mentioned a series of experiments carried on at Syracuse University, the purpose of which was to provide data which might aid in answering such questions as the following: Under controlled conditions, what per cent of individuals alter their expressions of opinion during discussion? Is the accuracy of individual judgments or opinions increased by discussion? Is the average opinion of the group more accurate after discussion than before? Is the typicality of the group increased during discussion? Are men more or less likely to change their opinions than are women? If there are changes, are they due to discussion itself or to other factors? Is there a relationship between willingness to alter opinion during discussion or to take the leadership in discussion groups, and standing in ascendance-submission, aggressiveness and mental tests?

It was also suggested that a topic to be discussed under experimental conditions should allow for (1) interest and comprehension on the part of the subjects, (2) diversity of opinion, (3) quantitative standards of measurement, (4) central point from which to measure dispersion, (5) sufficient range for expression of opinion so that no one might hold a more extreme opinion or judgment than is provided for in the scale of measurement. The present article will describe several experiments which were designed to furnish data bearing on the above-mentioned problems.

THE QUESTION EMPLOYED

In view of the practical difficulties involved in the use of attitude scales and topics of general importance, an extremely simple problem was used as the basis of discussion in this series of

* Manuscript received October 28, 1932.

[1] A more detailed account of the conditions and data of these experiments is to be found in Part I (pp. 21–82) of "Experimental Studies of Response to Social Stimulation," a Ph.D. dissertation on file at the Syracuse University Library. The writer is indebted to Professor F. H. Allport for suggesting much of the original setting of the experiments and for many helpful criticisms.

[2] This JOURNAL, 1932, 27, 29–34.

experiments. It concerned the number of beans in a sealed bottle. While such a question may seem to imply oversimplification and artificiality, nevertheless it satisfies the requirements previously mentioned. It allows quantitative statements of opinion and, by comparison with the facts in the case (the actual number of beans), provides measurements of the *quality* of judgments. The large number of beans in the bottle renders all judgments approximate (and therefore opinionative), due to the impossibility of calculating the exact number. The possible range of judgments is from zero to infinity, neither of which limits is likely to be closely approached. A considerable number of simple statistical techniques is applicable to data such as results from numerical judgments. A further advantage possessed by this question is that it involves little, if any, emotional bias.

SUBJECTS AND GENERAL PROCEDURE

The subjects who took part in this experiment were college students. They represent a highly selected group, restricted as to age, social status, intelligence and interests. These limitations should be respected in generalizing from the data which are to be presented.

For convenience, the 101 subjects will be described in four groups, as follows:

Group A—Twenty-six students in social psychology, 15 men and 11 women, classified as juniors, seniors and graduate students.

Group B—Twenty-four students in general psychology, 8 men and 16 women, classified as sophomores, juniors and seniors.

Group C—Twenty-three students in general psychology, 5 men and 18 women, classified as sophomores, juniors and seniors.

Group D—Twenty-eight students in general psychology, 23 men and 5 women, classified as sophomores, juniors and seniors, considered as a group for convenience, though they did not meet together. They were included as "controls" for purposes which will be explained later.

A number of sixteen ounce medicine bottles were prepared, each containing 811 small white beans. The bottles were corked and sealed with wax, so that the beans could not be removed. Three methods of presenting the question and controlling the discussion were followed, each of which will be described with the results tabulated separately. A fourth experiment, intended as a "control", in which no discussion took place, will also be described.

In every case the subjects made their first judgments in private in a room adjoining the office of the instructor of a class in which they were registered. They were under the impression that they were undergoing a test of their ability and that it would be desirable to make the best possible showing on the "test". This was done in order to promote interest in the question and to make sure that they took the matter seriously. All of the group meetings were held in university classrooms.

I. DISCUSSION IN SMALL GROUPS WITH KNOWN DIVERSITY OF OPINION.

The members of Group A took part in the first experiment. After all of the individual judgments had been made, the experimenter announced that the class would be divided into groups of three, so that each person might have the benefit of discussing the question with other individuals. The names of the persons constituting the various groups were read from a list previously prepared. Before the groups were assigned to the classrooms where the discussion was to take place, instructions were given that each committee was to carry out the following procedure: (1) elect a chairman, (2) decide on a number to represent the consensus of the committee, (3) return to the regular classroom where each member would write a report of what had taken place and also write his final judgment of the number of beans, regardless of what his committee had agreed upon as the correct number. This represents a weakness in method of experimentation, perhaps, because it called the attention of the subjects to the fact that their behavior was being watched closely. If the subjects had not been cautioned to remember what took place, however, the probability is that their reports would have been quite inaccurate.

As an incentive for a thorough consideration of the question, the instructor announced that the individual whose final judgment most nearly approximated the correct number of beans would be excused from writing up the experiment as a class exercise (which the other subjects were required to do), and in addition would receive a grade of "A" for that assignment. The same offer was made to the chairman of the committee which should report the group estimate most nearly approximating the true number of beans. The purpose of this latter offer was two-fold: (1) to afford an incentive for each subject to assert his ascendance in getting himself elected chairman of his committee, (2) to stimulate the chairman to greater effort in influencing the other mem-

bers of the committee toward a group estimate which seemed to him to be the most nearly correct.

The groups were then assigned to separate classrooms, where they were allowed to remain for fifteen minutes before returning to the regular classroom. Each group was provided with one of the 16 ounce medicine bottles containing 811 beans, to serve as a basis for discussion.

In dividing the class into groups, the committees had been "loaded", without the knowledge of the subjects, in two ways. In the first place, as great diversity of opinion as possible (based on the original judgments), was represented in each committee. This was to promote discussion, to make it more difficult to reach a conclusion and to prevent two members of the committee from combining against the third at the beginning. In addition to this, each committee contained one individual whose ascendance-submission score, as determined by the A-S Reaction Study,[3] was higher than the scores of the other two individuals. The purpose of this latter condition was: (1) to discover whether there was any relationship between the ascendance-submission score and the amount of change of opinion during discussion and (2) to discover whether the more ascendant individuals would be elected chairmen of their committees. The results showed little evidence of such tendencies, though the committee reports indicated a striking tendency for the individual standing highest in ascendance to take the lead in suggesting what the committee procedure should be.

As the A-S Reaction Study has two forms, one for men and one for women, with a scoring system using quite widely different numerical values, it was necessary to divide the group into committees on the basis of sex. The first five committees consisted of men and the other four of women. Committee IX contained only two members, as the number of subjects was not evenly divisible by three.

RESULTS AND COMMENT

The results of this experiment are summed up in Table I. It is to be regretted that the complete data cannot be presented, as in many cases only the whole distribution gives an adequate picture of the results. Many of the writer's generalizations are based on his knowledge of these distributions, rather than on the figures representing central tendencies, which may account in part

[3] Allport, F. H., and Allport, G. W. "The A–S Reaction Study in Personality." Houghton Mifflin Co., 1928. (Separate forms for men and women.)

for a lack of emphasis on probable errors in the discussion of results.

TABLE I

ESTIMATES OF THE NUMBER OF BEANS IN A BOTTLE BEFORE AND AFTER FACE TO FACE DISCUSSION: NINE COMMITTEES OF THREE MEMBERS EACH—CONSIDERABLE RANGE OF OPINION IN EACH COMMITTEE—ESTIMATES OF MEMBERS OF SAME COMMITTEE KNOWN—OTHER ESTIMATES UNKNOWN

GROUP A

Summary of Results

	Estimates			Error			Steps	Range in Committees		
	Before	After	Com'tee	Before	After	Com'tee	Changed	Before	After	Decrease
Average	847	772	775	305	122	91	312	694	125	569
Median	799	758	850	200	105	52	195	699	86	600
S. D.	412	135	106	279	69	62	296	376	127	395
Quartile dev.	225	106	100	150	45	54	178	220	46	421
P. E. Mean	±54	±18	±24	±37	±9	±14	±39	±84	±28	±89

Individual estimates improved..... 20
Individual errors increased..... 6
Range of whole group before.... 1875; after..... 474
Range in committee decreased in every case.
Average error of committee decreased..... 7; increased..... 2

Sex Differences:

Average estimate before—	Men	790	P.E. ±62
	Women.....	925	P.E. ±101
Average estimate after—	Men	695	P.E. ±18
	Women.....	878	P.E. ±16
Average number steps changed—	Men	256	P.E. ±48
	Women.....	382	P.E. ±62

While many of the data of Table I explain themselves, several items deserve comment. In the first place, both the mean and the median for the whole group are farther from the true number of beans after discussion than they were before. Taking these facts by themselves, one might hasten to the conclusion that discussion on a matter of fact does not seem to improve the accuracy of judgments. Certainly that is what results when one takes the *group* viewpoint, but when the statistics on individual members are taken into account it is evident that 20 of the 26 individuals improved their judgment. It will also be noticed that the average error was reduced from 305 to 122, or 60 per cent, following discussion. The figures for the medians, though somewhat smaller numerically, indicate the same tendency. The change was from 200 before to 105 after discussion, a reduction of 48 per cent. Probably just such situations frequently exist in communities where discussion of public questions does not seem to increase the accuracy of the "consensus" or average opinion; actually, however, the individual opinions of the members concerned may have increased considerably in accuracy during or after discussion.

The increase in typicality of opinion is quite marked, no matter what standard of judgment is employed. The range of the whole

group went from 1875 before discussion to 474 afterward, a decrease of 75 per cent. The range represented in each committee decreased in every case as a result of discussion. The average decrease of range per committee was 569 and the median of the committee decreases was 600. The quartile deviation or semi-interquartile range was lowered from 225 to 106, a reduction of 53 per cent. The standard deviation also indicated a decrease from 412 to 135, or 67 per cent.

The average number of steps changed is 195, or about 38 per cent of the number of beans in the bottle. This figure may be taken to represent the change of opinion quantitatively.

Calculations intended to bring out sex differences show that the average judgment of the women was 17 per cent higher than that of the men before discussion and considerably less accurate. After discussion, the average judgment of the women was 26 per cent higher than that of the men and considerably *more* accurate. The average number of steps changed by the women was 49 per cent greater than that of the men.

"INTELLIGENCE" AND CHANGE OF OPINION

A matter of minor interest was the relationship of "intelligence" to error of estimate, both before and after discussion. In Group A, the coefficient of correlation by the rank-difference method between the standings of the individuals in Morgan's Mental Test[4] and the errors of estimate before discussion was —.05 ±.13, while after discussion the coefficient was —.33 ±.12. Although the coefficients are too low for predictive purposes, the tendency seems to be for the duller individuals to make better judgments after discussion, *though their original estimates are approximately as accurate* as those of the brighter individuals. Were the second coefficient higher, one might suggest the generalization that the benefit derived from discussion varies inversely with intelligence, the less intelligent persons profiting most and the more intelligent persons profiting least. It is obvious, however, that these figures may describe only a chance occurrence. The number of steps changed seemed also to bear little relation to the factor of intelligence. The coefficient of correlation by the rank-difference method in this case is —.13 ±.13.

II. DISCUSSION IN SMALL GROUPS WITHOUT KNOWLEDGE OF THE WIDELY DIFFERING OPINIONS OF OTHERS.

Approximately one year after the experiment in which the advanced students took part, a somewhat similar procedure was

[4] Morgan, J. J. B. "Morgan's Mental Test", Form A. Iowa City, Iowa, The Clio Press, 1922.

employed with a group of beginning students. The same bottles were used, each containing 811 beans and the initial judgments were made in a room adjoining the office of the instructor, as in the previous experiment. At the close of the proceedings, the subjects were asked whether they had ever heard of a similar test. Their unanimous negative reply was taken to indicate that there was no carry-over of information from the previous undertaking.

In the second experiment, the selection of committees and the committee procedure itself differed from the conditions prevailing in the first experiment. In dividing the members of Group B into discussion units, four members constituted a committee, instead of three, as formerly; no attention was paid to the personality traits of the subjects, nor were the women segregated from the men. Another condition peculiar to the experiment with Group B was that each committee, instead of representing fairly wide diversity in original estimate, represented as close agreement on the initial judgment as was possible. Committee I, for example, was composed of the four subjects whose first estimates were the lowest in the class. Committee II contained those whose estimates were next higher and so on, until Committee VI represented the four highest initial judgments. This "loading" was carried out deliberately in an attempt to bring about conditions as nearly as possible the antitheses of conditions in the experiment with Group A. In the latter experiment the mere *knowledge that others differed, apart from the discussion itself,* might be expected to cause the individual to modify his judgment. It is at least conceivable that the individuals might have changed their estimates fully as much if they had simply read about the estimates made by others, without coming into face to face relationship for discussion at all. In other words, where there is no indication that others differ in opinion, discussion may not necessarily bring out any further knowledge of the question and consequently may not stimulate a change of opinion.[5] This situation is further complicated by the fact that what Professor Allport calls the *impression of universality*[6] might be expected to occur under the circumstances. For example, four subjects, meeting in a group and finding that they differed only slightly in their estimates, might conceivably come to labor under the delusion that since they represent a fair sample of the class, the probability is

[5] In commenting on this experiment, Professor Allport has suggested that in many of the previous generalizations concerning the effect of discussion on change of opinion, too much emphasis has been placed on the effect of discussion and too little emphasis on the fact that the real stimulus for change may lie in this knowledge that the estimates or opinions of other individuals differ from one's own estimate or opinion.

[6] *Social Psychology,* 1924, p. 306.

that every one in the class, or nearly every one, believes as they do. If it may be assumed that the other individuals have made about the same estimates, it is only reasonable under the circumstances to assume that the judgments are fairly correct. A weakness in the method of experimentation is that no check was provided to ascertain whether or not this impression of universality really did prevail. From his observation of the committees in action, the experimenter believes the impression of universality was present, but he has no definite proof of its existence.

The adoption of these conditions also served as a check on any generalizations regarding change of opinion as a result of discussion which might be made from an experiment such as the preceding one. This was deemed necessary, because it seemed probable that outside the laboratory the conditions under which discussion takes place may more nearly approximate those which prevailed in Group B, rather than those exercised in the case of Group A. In numerous instances, at least, when a question is discussed generally throughout a community, it is taken up mainly in small groups of persons whose opinions agree at the start. This fact should be recognized in laying out experimental situations.

RESULTS AND COMMENT

The numerical data from this experiment are included in Table II.

In one respect, the results shown in Table II antithesize to a certain extent the data of Table I, where neither the average nor the median of the whole group showed improvement, while 77 per cent of the individuals improved the accuracy of their judgments. Table II indicates that for Group B the average after discussion is 39 closer to the correct number of beans than it was before, representing 85 per cent improvement, and the median is 26 closer afterwards, an improvement of 59 per cent. However, only half of the individuals improved their personal judgments, while one-third of them decreased the accuracy of their estimates and one-sixth made no change at all. Both the average and median number of steps changed in this experiment are considerably less than the corresponding figures for Group A, and for those of Group C, which are to be presented in Table III. The average error was also decreased less in the case of Group B than it was in the other groups. Table I shows a reduction from 305 to 122, or 60 per cent, as compared with the reduction from 261

to 217, or 17 per cent, indicated in Table II. Figures to be presented later in Table III indicate a change in average error from 465 to 279, a reduction of 40 per cent. A comparison of the medians of the errors indicates the same tendency shown by the means. Table I shows a reduction from 200 to 105, or 48 per cent and Table III indicates a decrease from 304 to 111, or 63 per cent. In the present case, as shown in Table II, the reduction amounts to 30 per cent.

TABLE II

ESTIMATES OF THE NUMBER OF BEANS IN A BOTTLE BEFORE AND AFTER FACE TO FACE DISCUSSION: SIX COMMITTEES OF FOUR MEMBERS EACH—SLIGHT RANGE OF OPINION IN EACH COMMITTEE—ESTIMATES OF MEMBERS OF SAME COMMITTEE KNOWN—OTHER ESTIMATES UNKNOWN

GROUP B

Summary of Results

	Estimates			Error			Steps	Range in Committees		
	Before	After	Com'tee	Before	After	Com'tee	Changed	Before	After	Increase
Average	857	804	820	261	217	168	225	162	258	97
Median	875	773	858	237	166	129	149	100	155	33
S. D.	313	279	213	178	178	131	209	107	353	228
Quartile dev.	216	161	89	97	101	115	132	58	108	85
P. E. Mean	±43	±38	±59	±25	±25	±36	±29	±29	±79	±63

Individual estimates improved					12
Individual errors increased					8
Individual estimates unchanged					4
Range of whole group before 1190; after					1265
Range in committee decreased 2; increased 3; unchanged					1
Average error of committee decreased 4; increased					2
Sex Differences:					
Average estimate before—	Men	767	P.E.	±92	
	Women	902	P.E.	±45	
Average estimate after—	Men	662	P.E.	±58	
	Women	875	P.E.	±52	
Average number steps changed—	Men	172	P.E.	±36	
	Women	251	P.E.	±30	

It is also evident that the total range of opinion was increased by 75 steps, or 6 per cent. The other measures of dispersion, however, indicate an increase of typicality, the quartile deviation being changed from 216 to 161, a reduction of 25 per cent, and the standard deviation from 313 to 279, slightly less than 11 per cent. While the evidence here is not entirely reliable, it points strongly to the conclusion that the absence of knowledge that other individuals differ, while it does not militate against improvement in accuracy of the consensus as judged by the average or median of a group, nevertheless does tend to decrease individual change of opinion and individual improvement in accuracy and to prevent narrowing the range of opinion during discussion regarding a matter of fact. This conclusion is further enhanced

by the indication that the average range of opinion within the committees is not decreased by discussion, as was the case in Groups A and C, but that it is increased. Three of the committees increased their range, two decreased it and one made no change in its range. In the other experiments no committee failed to decrease its range, the average decrease in the other experiments being approximately six times the average increase shown in Group B.

What the effect would be in case of a question of opinion, instead of a matter of fact, might afford considerable speculation. Under such circumstances, the probability is that among members of small groups, absence of the knowledge that others differ might tend to make discussion strengthen the opinions originally held, rather than to change them to any extent.

The data indicating sex differences are remarkably similar to the corresponding figures in the preceding experiment, in spite of the fact that the sexes were not segregated in the case of Group B, while in Group A they were. In the present case, the average estimate of the women was 18 per cent higher than that of the men before discussion, 32 per cent higher after discussion, and considerably more accurate after discussion than that of the men, even though the average of the women was less accurate than that of the men before discussion.

III. DISCUSSION AMONG CHANCE GROUPS IN A LARGE MEETING

The individuals of Group C took part in the third experiment. Fortunately for the experimenter, a situation arose in the classroom, shortly after the individual judgments had been made, which brought on what seemed to be a spontaneous discussion of the question. On being asked to do so, the experimenter read aloud the names of the subjects and their initial estimates. With this knowledge, the subjects were given opportunity to discuss the question for thirteen minutes (two minutes having been consumed in reading the estimates, etc.). Eight of the bottles were issued to members of the class as they sat in the classroom. Several of the subjects changed seats, so as to gain closer observation of the bottles. In this way were formed "natural" committees, of from two to four members each, probably better suited for purposes of discussion than the "loaded" committees of the previous experiments. While the discussions were carried on aloud, so that the subjects might hear what members of other groups said, most of the discussion took place within the committees,

rather than between committees. At the close of the experiment each subject wrote his final secret judgment on a piece of paper which was given to the experimenter.

RESULTS AND COMMENT

In Table III the statistical devices employed in dealing with the results of previous experiments have been applied to the present data, even though the committees were not of uniform size.

TABLE III

ESTIMATES OF THE NUMBER OF BEANS IN A BOTTLE BEFORE AND AFTER FACE TO FACE DISCUSSION: SUBJECTS PRESENT IN ONE LARGE GROUP BUT WORKING AS EIGHT COMMITTEES OF FROM TWO TO FOUR MEMBERS EACH, ACCORDING TO CHANCE GROUPINGS—ESTIMATES OF ALL OTHER SUBJECTS KNOWN

GROUP C

Summary of Results

	Estimates		Error		Steps	Range in Committees		
	Before	After	Before	After	Changed	Before	After	Decrease
Average	977	974	465	279	281	1079	530	549
Median	750	750	304	111	200	717	50	580
S. D.	891	772	777	738	262	1110	1177	291
Quartile dev.	337	125	217	38	225	298	100	232
P. E. Mean	±125	±109	±109	±104	±37	±265	±281	±69

Individual estimates improved..... 17
Individual errors increased..... 5
Individual estimates unchanged..... 1
Range of whole group before.... 4575; after..... 3886
Range in committee decreased in every case.
Average error of committee decreased..... 5; increased..... 3

Sex Differences:

Average estimate before—	Men	870	P.E. ±85
	Women.....	1007	P.E. ±158
Average estimate after—	Men	816	P.E. ±58
	Women.....	1018	P.E. ±137
Average number steps changed—	Men	155	P.E. ±44
	Women.....	316	P.E. ±44

In general, the data for Group C resemble those for Group A more closely than they do those for Group B. This is probably due to the fact that in Group C, as in the case of Group A, each subject knew that other subjects differed considerably from his own estimate. The average and median estimates of the group show strikingly little change, however. It is here, more nearly even than in the preceding experiments, that the danger of generalizing regarding the improvement in consensus of a group as indicated by the average or the median is plainly to be seen. Although the average judgment is not significantly nearer the correct number of beans, 17 subjects, or 74 per cent of the whole group, improved their judgments, while 5 subjects or 22 per cent

decreased in accuracy. Only one subject failed to change his estimate.

The average error was decreased from 465 before discussion to 279 afterward, an increase in accuracy of 40 per cent. The median error also indicates a considerable increase in accuracy, being reduced from 304 to 111, or approximately 63 per cent. The range of the whole group was reduced by only 15 per cent, but the extremely large range represented here is plainly due to an enormously high estimate made by one of the women. Here the true reduction in dispersion is probably better indicated by the change in quartile deviation, which was reduced from 337 to 125, or 63 per cent. The standard deviation was 891 before discussion and 772 afterward, the large figures being due no doubt to the one extreme estimate mentioned above. Every "committee" showed a considerable decrease in dispersion, the average decrease being 549 and the median decrease being 580. Here again the medians of the ranges represented in the "committees" before and after discussion, 717 and 50, respectively, are probably better criteria than the means, because of the extreme effect caused by the one high estimate previously mentioned.

The data indicating sex differences are quite similar to those recorded in the previous experiments. The average judgment of the women was 16 per cent higher than that of the men before discussion and 25 per cent higher after discussion, while the average number of steps changed among the women was 104 per cent higher than that of the men. In this experiment, however, the average judgment of the men was more accurate than that of the women after discussion, a condition dissimilar to the one prevailing in the previous experiments.

IV. A CONTROL EXPERIMENT

In presenting the data of these experiments it has been assumed that the changes in estimates made by the subjects were largely due to the type of discussion which resulted in the awareness that others held different opinions. A serious objection to this assumption is the fact that many of the changes may have been due to the opportunity to make a second observation of the bottles of beans, rather than to any influence present under the conditions of discussion. In order to ascertain how much of this change might have been due to second observation, a control experiment was carried out with the members of Group D as subjects. Here the subjects were asked to make two estimates with no opportunity for discussion.

RESULTS AND COMMENT

The data from the control experiment appear in Table IV. As these individuals did not meet in groups, several of the columns which appear in the preceding tables are necessarily absent there.

TABLE IV

FIRST AND SECOND INDIVIDUAL ESTIMATES OF NUMBER OF BEANS IN A BOTTLE WHEN NO DISCUSSION TOOK PLACE: EACH INDIVIDUAL ALONE—NO KNOWLEDGE OF ESTIMATES OF OTHERS

GROUP D

Summary of Results

	Estimates		Error		Steps
	First	Second	First	Second	Changed
Average	992	1019	338	323	188
Median	916	925	167	160	150
S. D.	588	496	514	430	177
Quartile dev.	166	197	98	128	85
P. E. Mean	±75	±63	±66	±55	±23

Individual estimates improved				16
Individual errors increased				11
Individual estimates unchanged				1
Range of whole group first time	2925;	second time		2356
Sex Differences:				
Average estimate, first—	Men	1034	P.E.	±89
	Women	797	P.E.	±68
Average estimate, second—	Men	1012	P.E.	±73
	Women	1049	P.E.	±120
Average number steps changed—	Men	175	P.E.	±18
	Women	252	P.E.	±95

Table IV shows the mean of the second estimates to be slightly farther from the actual number of beans than is the mean of the first estimates, indicating a loss in accuracy of approximately 3 per cent. The median is 1 per cent less accurate. The average error is reduced from 338 to 323 and the median error from 167 to 160, only 4 per cent in both cases. This is considerably less than is to be found in the other tables, with the possible exception of Table II, which indicates a reduction in mean error of 17 per cent and a reduction in median error of 30 per cent. For Groups A and C, respectively, the mean errors were reduced 60 per cent and 40 per cent; the median errors, 48 per cent and 63 per cent. It will also be noticed that in the control group 16 individuals (57 per cent) improved the accuracy of their estimates, while 11 (39 per cent) decreased their accuracy and the judgment of one (4 per cent) remained the same. In Groups A and C, respectively, 77 per cent and 74 per cent of the individuals improved their judgments; in Group A, 23 per cent decreased the accuracy of their estimates; in Group C, 22 per cent decreased the accuracy

of their estimates and 4 per cent made no change. In Group B, where the subjects had opportunity for discussion, but did not know how greatly the others differed, 50 per cent of the individuals improved their judgments, 33 per cent reduced their accuracy and 17 per cent made no change.

This may be taken to indicate that the increase in accuracy of individual judgments shown in the case of Groups A and C is due largely to the fact that the subjects knew how the estimates of others differed from their own. Evidently the opportunity to make a second choice had little to do with the improvement. The effect of discussion in increasing accuracy of individual estimates seems to be negligible, as the results where discussion took place in the absence of knowledge of the differing judgments of others are quite similar to those obtained in the control group.

It must be remembered, moreover, that the number of individuals making no change was greatest in Group B. Were the number of cases involved not so small, the conclusion might be drawn that discussion in the absence of awareness of differing judgments tends to *increase* the strength of the opinion originally held, rather than to change it. The impression of universality may perhaps have been partially responsible for the latter tendency, but of course this is not definitely known.

The total range was changed from 2925 to 2356 by virtue of the second judgments, a reduction of 18 per cent, which corresponds quite closely to the 15 per cent reduction shown in Group C. It is interesting to note that while the standard deviation was decreased from 588 to 496, or 16 per cent, the quartile deviation was increased from 166 to 197, or 19 per cent. This seeming incongruity is explained by the fact that the estimates of two individuals were extremely large in the first place and that the reductions in the squares of the deviations were sufficiently large to account for the change. The true state of affairs is perhaps better indicated by the increase in the quartile deviation, rather than by the reduction of the standard deviation. In any case, it is safe to propose the generalization that the rather large increases in typicality, as measured by decrease in standard deviation and quartile deviation, to be found in Tables I, II, and III were due to the effect of the awareness of the judgments of others, rather than to the opportunity to make a second estimate.

It will also be noticed that the quartile deviation of the errors is increased from 98 to 128, or 31 per cent. In Group A the quartile deviation of the errors was reduced from 150 to 45, in

Group C from 217 to 38; in Group B, where the individuals were not aware of the differing opinions held by others, the quartile deviation of the errors was increased from 97 to 101.

Inspection of both the mean and median number of steps changed in all the tables shows these figures for the control group, 188 and 150, respectively, to be much lower than the corresponding figures for Groups A and C, but almost comparable to the figures for Group B. The table for the latter group shows the mean to be 37, or 20 per cent higher than the mean for the control group, but the medians of 150 and 149 are practically the same.

The data on sex differences are not particularly reliable, as only 5 of the 28 subjects in the control group were women. It will be seen, for example, that the average estimate of the men is considerably higher than that of the women on the first estimate, which is not the case in the previous tables. However, the average of the second estimates of the women is higher than the average for the men, as is the case in all previous tables. The average number of steps changed by the women is 252, which is 44 per cent higher than the average of the men. These figures agree rather closely with the figures for the other groups, with the exception of the fact that in Group C the women changed 104 per cent more than the men. The average for the men in Group C is lower than usual, however, and not very reliable, as there were only 5 men in the group.

OBSERVATIONS FROM THE COMBINED DATA

If the estimates of the individuals in Groups A, B and C are combined, it is evident that 93 per cent of the judgments were changed after discussion. This is approximately the same as the the result obtained in Group D, where no discussion took place, but 96 per cent of the individuals changed their estimates. If Group B, where the subjects were not aware that others held widely differing opinions, is left out of the calculation, it is seen that 98 per cent of the individuals changed their estimates after discussion.

There is little evidence to show that discussion makes the average or the median of the estimates more accurate, but when the results of Groups A and C are combined, it is seen that 76 per cent of the individual estimates are improved, 23 per cent are decreased in accuracy and 2 per cent remain the same, where the subjects are aware of the differing opinions of others. As compared with the corresponding figures for the control group, 57 per cent, 39 per cent and 4 per cent, respectively, the differ-

ence is significant. The latter figures more nearly resemble those of Group B, where the figures are 50 per cent, 33 per cent and 17 per cent, respectively. It seems safe to draw the tentative conclusion that the lack of awareness of the differing opinions of others during discussion tends to decrease improvement of estimate to a degree lower than the usual tendency to change of one's own accord.

When the data of Groups A, B and C are combined, the average estimates of the women are 950 and 733, before and after discussion, respectively. The corresponding figures for the men are 798 before, and 707 after discussion. This may be interpreted as an indication of the influence of social pressure in contributing to understatement, rather than overstatement of opinion, even after the individuals who constituted the social situation are no longer present.

The men of Groups A, B and C showed an average change of 214 steps, while the corresponding figure for the women was 309. In all of the tables this same tendency for the women to make greater changes than those made by the men is evident. There seems to be little evidence that discussion is a factor influencing this relationship, as the women exceed the men in average number of steps changed, whether discussion has taken place or not.

SUMMARY

(The following summary must not be regarded as a list of final conclusions, if for no other reason than that rather large probable errors have been disregarded in treating the data. There is reason to believe, however, that data similar to those presented here will result whenever the same experimental conditions prevail.)

1. When a considerable number of persons entered into discussion of a question of fact, either in a large group, or in several smaller groups, and learned that other individuals differed materially in their opinions regarding the question, the following results occurred:

   (a) The *number of individuals* who did *not* alter their opinions was negligible. However, approximately the same number of changes occurred in a control group where the subjects were asked to make two estimates *without* opportunity for discussion.

   (b) The accuracy of the *average judgment* of the individuals was not improved.

(c) The number of individuals who *increased* the accuracy of their estimates was approximately three times the number who did not improve their estimates. (This latter number includes those who *decreased* the accuracy of their judgments *and* those who made no change.) By comparison, where no opportunity was allowed for discussion, approximately half of the individuals increased the accuracy of their estimates by a second judgment.

(d) The *decrease in average of the individual errors* was greater than when no opportunity was allowed for discussion.

(e) The *typicality of opinion* was increased, *i.e.,* the dispersion, as indicated by the range, quartile deviation and standard deviation, was decreased.

(f) With the subjects divided into small groups, *the range of opinion* in each and every group was reduced.

(g) The *average amount of individual change* was greater than where no opportunity was allowed for discussion.

2. Where the individuals entered into discussion without becoming aware that others held noticeably differing opinions, most of the effects mentioned in c, d, e and f, above, did not occur. *I.e.,* in the absence of awareness of difference on the part of other individuals:

(a) The number of individuals who improved the accuracy of their judgments was no greater than the number who improved their accuracy under solitary conditions.

(b) More individuals showed no change of opinion than was the case under any other conditions in these experiments, not excepting the control group.

(c) The average of the individual errors was decreased less than under other conditions of discussion.

(d) The usual tendency to reduce the range was absent, both as regards the range of the entire group and the several ranges of the smaller groups.

3. Under each and all of the conditions prevailing in these experiments the average change of opinion was greater among women than among men.

4. The data indicate a general tendency for individuals to express more "conservative" opinions (*i.e.,* to lower their estimates), as a result of committee contacts.

5. Little evidence was found to show that intelligence, ascendance-submission or aggressiveness, as indicated by test scores, did or did not bear a significant relationship to willingness to change opinion, or to election to chairmanship of a committee. There was a slight tendency for the less intelligent to profit most from the discussion and for the more ascendant individuals to initiate and direct proceedings, though not necessarily to achieve chairmanships.

The most significant general conclusion from these data is that discussion is not effective in changing opinion unless the individuals who enter into discussion become aware of differences in opinion held by others.

THE NATURE OF EMOTION

By JAMES DREVER
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IT IS somewhat unpleasantly surprising to find that psychologists are still so far from agreement as to how emotion is to be treated in a systematic psychology, whereas in regard to the descriptive psychology of emotion there is practical unanimity. Part of the trouble is doubtless due to the fact that psychology is still affected by philosophical controversies. The conflicting views regarding the manner in which emotion ought to be treated are apparently the echo of the old body and mind controversies. Before any agreement is possible we must get rid of this influence, which is an entirely illegitimate one. The concrete fact for the psychologist is the living organism; body and mind are both abstractions. It must of course be recognized that an emotion has two aspects. On the one hand it is a phase of the response of the organism to its environment; on the other hand it is a type of experience. These two aspects, response and experience, are both in the concrete phenomenon. It is when we regard them as bodily and mental respectively, that we are in danger of abandoning the concrete for abstractions, and of opening the way for the numerous difficulties and perplexities, with which the systematic psychology of emotion is so entangled.

There is, as I have said, practical agreement with respect to the concrete phenomena on both aspects. As response emotion involves a widespread disturbance of motor and glandular functions, the different phases of which can be for the most part objectively observed. As experience emotion involves a mass of sensuous and affective elements with more or less characteristic impulsions, all referred to a more or less clearly apprehended situation, which in turn can be introspectively examined. There is introspective evidence also that emotions are characterized by a narrowing of consciousness, which is best interpreted in terms of dissociation, and which manifests itself in two ways—on the one hand in what may be called a specificity of appeal, so that certain objects and certain actions alone have what might figuratively be termed right of entry in experience, and on the other hand in a marked diminution of rational control, so that in extreme cases

the sentiments and principles of a life-time may be shed, and primitive man, eoanthropus an actuality, may stand revealed.

Up to this point practically all psychologists are in full agreement. A large measure of agreement would also probably be secured for a systematic interpretation of the phenomena, at least on the response aspect, in physiological terms. We can regard emotion as involving, in varying degrees according to the intensity of the emotion, a diminution of cortical control, amounting in extreme cases to its virtual abolition, so that, as Paton has put it, the old brain takes charge to a varying extent. The response of the organism becomes as a result in varying degrees protopathic, to use Head's very useful term. We can carry this type of interpretation and explanation even farther. As we have seen the dissociation in emotion may be either specific or general. In virtue of specific dissociation those ideas, attitudes, and acts, which are not relevant to the impulse involved, lose their appeal. This might be interpreted physiologically as a dissociation within the cortex itself. In virtue of general dissociation all responses become, as we have seen, protopathic. This might be interpreted as dissociation between the cortex and the lower centers—presumably thalamic.

Agreement, however, does not go beyond this. As soon as we attempt a systematic interpretation of emotion in psychological terms, difficulties and controversies immediately make their appearance. In recent times the controversies have largely centered around McDougall's attempt at a systematic psychology. Full credit must be given to McDougall for this attempt. He is probably in error in several respects, but it may be that his errors point the way to a satisfactory solution of the difficulties. It is evident that an attempt to relate emotion to instinct is likely to lead only to confusion if we emphasize the experience aspect of emotion and neglect the response aspect, still more if we emphasize the affective in emotional experience and ignore the rest. McCurdy, one of McDougall's chief critics, comes to somewhat similar conclusions, though along a different line of thought, which leads him to a view of what he calls "affect" that is, I believe, in the last resort untenable as a scientific view, at least as untenable as the view he is criticising. It appears to involve an absolute separation of the two aspects of emotion, which are after all two aspects of one and the same concrete series of phenomena.

Let us examine the focus of controversy more closely. The term "instinct" has in recent times given rise to a great deal of discussion, and there has been a great deal of difference of

opinion as to the exact meaning to be attached to it. Nevertheless the original and essential meaning of the word is not in doubt. *Instinctus* was the word employed by the Romans to translate the Greek *horme* (ὁρμή). The root idea of the Greek word is "drive", "urge", "impulsion". The word was employed by the Stoics in the sense in which "instinct" has been rightly used by writers in modern times from the seventeenth century onwards. A secondary and subsidiary sense of the word is of course "a specific form of behavior congenitally determined", but this *is* secondary and subsidiary. In the light of these facts it is difficult to understand the widespread movement at the present time in favor of abandoning the term "instinct" altogether as far as psychology is concerned, and when necessary using the term "drive" instead. If "drive" designates a real psychological phenomenon—as it undoubtedly does—why not continue to speak of "instinct" as before in the case of "drives" which are characteristically congenital and universal?

In the concrete an "impulsion" or "drive", like emotion, has two aspects. On the one hand it is a phase of experience—conation in fact. On the other hand it expresses itself in behavior. Hence the secondary and subsidiary meaning of "instinct". The question therefore necessarily arises: How are instinct and emotion related to one another? The view that important human emotions are closely related to fundamental human instincts or impulsions has behind it a consensus of psychological opinion from the earliest times downwards, that is little short of astonishing in view of some of the discussions that have been going on in psychological circles recently. Few writers stop to argue the point. Almost all assume that, as James puts it, "every object that excites an instinct excites an emotion as well".

It was McDougall's definite formulation of this relation as what we may call a simple part-whole relation that loosed the floods of controversy during the present century. It may be worth while attempting a reformulation of the relationship on the basis of a criticism of McDougall. According to McDougall's formulation an emotion is the characteristic and stable part of the activity of an instinct, which latter, as we all know, he defines in terms of structure—psychophysical disposition—rather than in terms of event or process. This means in effect that for McDougall an emotion is an instinct in operation regarded in its affective aspect. Now something may be said for this formulation, if we are prepared to give the term "emotion" a technical meaning in psychology which is markedly different from its meaning in popu-

lar speech. If we are not so prepared the formulation cannot be accepted. The objection most frequently urged against McDougall's theory is that instincts may operate without any significant emotional perturbation. That is, it is not the case that instinctive activity always involves a characteristic emotion in the ordinary sense of "emotion". This point has been most clearly and convincingly put by Rivers. "Manipulative activity", he says, "in response to danger may be wholly free from fear, or from any other emotion, except perhaps a certain degree of excitement", and it is a general law that "manipulative activity" and "absence of affect" tend to go together.

I have elsewhere[1] urged against McDougall's view what seems to be a more important, and indeed fundamental, objection. Even when emotion is present, the situation is not so simple as McDougall's theory would lead us to suppose. Not only so, but in McDougall's own account of instinct emotions there is a serious inconsistency, which it is very surprising that he himself has apparently never recognized. Compare the instinct of flight or escape and the instinct of self-display. According to McDougall's account the characteristic emotion in the former case is fear, in the latter elation. But fear is evoked in proportion as avenues of escape are cut off, in proportion, to put it briefly, as the impulse to escape is thwarted or obstructed, whereas elation is experienced in proportion as the impulse is successful, in proportion as the individual meets the admiring regards of other people. If other people ridicule, in place of admiring, instead of elation the emotion experienced is that which we colloquially describe as "feeling small". Hence "feeling small", and not elation, bears the same relation to the self-display impulse as fear to the flight impulse. But the flight impulse may also be facilitated in place of being thwarted. We then experience the emotion we call "relief". Thus in each case we have two emotions, and a closer examination of the other instinctive impulses listed by McDougall will reveal similar phenomena in every case.

We are thus led to a characteristic of our emotional life which has never received the attention it merits. This characteristic may be designated "bipolarity". In the same way as simple feelings range between two opposite poles of pleasure and unpleasure, so emotion ranges between two opposite emotional poles. One of these poles we would all agree in designating "joy". "Sorrow" the opposite of "joy" usually has a rather restricted

[1] **Instinct in Man, 2nd ed. Appendix, and Introduction to the Psychology of Education.**

meaning in popular speech. Nevertheless I do not think it would be illegitimate to widen this meaning, and to speak of the two polarities of emotion as "joy" emotion and "sorrow" emotion respectively.

To come back to the general question, how are we, in the light of these facts, to describe the relation of emotion to instinct? The position would appear to be this. The evoking of an instinctive impulse need not involve anything we can or ought to call emotion. Such elementary feeling as it does involve is most fittingly called "interest". In proportion, however, as the impulse is thwarted the response of the organism becomes increasingly protopathic, there is emotional dissociation, and we experience emotion of a "sorrow" polarity. The "joy" polarity is neither so frequent nor usually so intense. This develops when, and in proportion as, the impulse is facilitated, more particularly if the facilitation is sudden or unexpected, or if facilitation unexpectedly takes the place of obstruction. The result again is protopathic response, emotional dissociation, emotional experience.

But at least two more general principles of interpretation also emerge. In the first place it is clear that emotion is not a definite and simple psychological entity at all. It is a phase at once of response and of experience, and complex on both aspects. We may speak of emotion in general, but ought not to speak of "an emotion". Or one might be inclined to agree with McCurdy's contention, and with what appears to be McDougall's latest view, that the term should be employed adjectivally rather than substantively as a psychological term. That is, we should speak of emotional response and emotional excitement, rather than of emotion. I think, however, that McCurdy is deluding himself if he believes, as he apparently does, that the separation of affect and emotional response or expression is a solution of any of the difficulties. In the second place emotional excitement of either polarity, and emotional response, may accompany any impulse or conation, whether simple or complex, provided the appropriate conditions are present. Complex impulses and conations can possibly be analyzed into, or traced back to, instinctive impulses, but a one to one relation of instinctive impulse to emotion does not exist.

A SYSTEMATIC TRANSLATION OF PSYCHOANALYTIC CONCEPTS

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IT is to be granted to begin with, that certain portions of the psychoanalytic literature are not to be lightly discarded.

(1) A large number of phenomena occurring in intra-family relationships, in infant life, and in the development of sex behavior, have been brought to light. The observation and report of obscure phenomena is an unassailable contribution, although a tendency toward unwarranted generalization has detracted from its value; as when the castration complex is arbitrarily bestowed upon all little boys; as when all sculpture is attributed to coprophilia or as when a universal pattern of sex development is worked out for all children.

Nevertheless, the mental events and behavior associated with the castration complex and coprophilia are significant elements in the lives of many individuals.

(2) The technique of the social study or the clinical investigation has borrowed largely from the technique of psychoanalysis. The clinical study—the collection and interpretation of data in the history of an individual, and their application to his present behavior—is today an indispensable tool in psychological research and in case study. The analysis of the origins of a symptom could attain the scientific standard of the case study of the social worker or the school psychologist, but for an incredible bias as to the nature of the conclusions.

(3) In therapeutic virtue, psychoanalysis compares very favorably with prayer or the laying on of hands. And it can be shown that there is more than mere suggestion to explain its effectiveness, at least when the analysis happens to be correct.

So much of value, then, we must grant. When, however, we come to the systematic structure of psychoanalysis, we find very little which we can justify. We find, in fact, not a single consistent theory, but a series of successive deposits of elaborate theories, impossible to fuse into one.

The time is past when the acute perception which the psychoanalysts have undoubtedly displayed will induce us to swallow

their naïve theorizing and their fancy-free invention of hypotheses and "mechanisms". Their data of observation constitute a brilliant contribution; their fantastic hypotheses are an obstacle to scientific progress.

This is the first part of an attempt to interpret the observable facts of abnormal behavior in terms of laboratory psychology, with stress upon the laws of learning and the stimulus-response formula. The essential terms of the psychoanalytic theory will be taken in order for reinterpretation.

Attempts which have been made to date to describe the mechanics of neuroses and psychoses may be classified in three groups:

(1) Descriptions based upon some single ill-defined but quasi-scientific master-principle, such as *poverty of mental energy*, *dissociation*, or *suggestion*.

It is probably assumed in such theories that the key-concept is thoroughly understood to begin with, since no attempt is made to describe it in terms of any comprehensive system. Hollingworth (5, Chapter 14) has pointed out that such items can be extended to explain many or all normal phenomena, and hence mean nothing as to the peculiar cause of abnormality. Furthermore, they do not account for the susceptibility of certain individuals to abnormal symptoms.

The essential point is that all men have formed their habits similarly, whether normal or abnormal, and that no special mechanism is required to explain abnormal reactions. If we have arrived at, or have reasonably well approached, an adequate account of habit-formation, then we must dispense with any alternate theory.

(2) Descriptions patterned more or less after the Freudian terminology are by far the commonest employed by psychopathologists; and properly enough, since the conflicting schools of the academic theorists have produced no set of concepts nearly as convenient. The *complex*, the *repressed wish*, the *unconscious desire*, the *conflict*, and so forth, are the terms with which all workers, whether in analysis, psychiatry, or social welfare, whether in agreement with the peculiar beliefs of the Freudians or not, have been best able to discuss the causes of psychological maladjustment.

For the academic psychologist, these terms are too bizarre and too remote from orthodox technical terminology. Obviously the mind of the subject in the laboratory works in the same manner

as the mind of the neurotic. The two should be described in similar terms.

It is a notorious fact that the laws and generalizations of psychoanalysis have been issued by a few men in the manner of prophets, and have been accepted on pure faith. We cannot accept as omniscient, or reliable, individuals whose pronouncements have often shown incredible naïvete or a flagrant disregard for the law of parsimony.

It is declared, for example, that the sexual libido, when unexpressed, turns into free-floating fear and attaches itself to a symbolic object to form a phobia. Are we not justified in demanding a survey and statistical confirmation of this generalization, objective demonstration of the possibility of such a conversion, and a clear definition of terms?

(3) Until recently, no attempts were made to incorporate the facts of abnormal psychology within the theories elaborated by any of the systematic schools.

That such a situation should exist speaks ill for the state of academic psychology. When conflicting schools may co-exist within one science, it is obvious that no fully adequate picture of mental life has actually been evolved. Small wonder, then, that for thirty years the facts of psychoses and neuroses have been handled in obscure and mystic terms.

We might expect McDougall, interested as he is in the unorthodox but fairly comprehensive system with which his name is identified, to make short work of the Freudian system. What is our surprise, then, to see that brilliant and sophisticated polemic embrace large portions of the theory and its terminology, gravely deliberating whether "conduct without repression can give rise to a condition that may be properly called one of neurotic disorder" (12, p. 217), or whether a "conflict resulting in repression may run its course wholly subconsciously" (12, p. 222).

Here, it is obvious, we can have no clearing of gratuitous hypotheses, or simplification of terminology. The new account is, in fact, so extended that we cannot undertake a detailed criticism. We must take refuge in the statement that the hormic psychology can offer little hope of establishing harmony, when it takes sides sharply against many present-day developments.

Let us pause to make one point by way of demonstration: Recent observations show that the growth of the nervous system in utero, and the acquisition of reflex-responses before birth, are processes very similar to learning (8, pp. 20, 37). In the face of such facts, how seriously shall we consider a *submissive instinct?*

It has often been asserted that the principle of the conditioned reflex will suffice to explain the phenomena of abnormality. For example, Watson (14, pp. 297, 300):

> "I venture to predict that twenty years from now an analyst using Freudian concepts and Freudian terminology will be placed upon the same plane as a phrenologist. And yet analysis based upon behavioristic principles is here to stay" . . . "I can only roughly outline my own viewpoint. Sickness of personality, or behavior illness, behavior disturbances, habit conflicts, are terms which I should like to use in place of mental disturbances, mental diseases and the like." . . . "I admit the exaggeration but I am after elementals here. I am pleading for simplicity and ruggedness in the building stones of our science of behavior. I am trying to show by this homely illustration (of a hypothetical dog rendered 'insane' by conditioning) . . . that you can by conditioning build up the behavior complications, patterns, and conflicts in diseased personalities."

The fact remains that no thorough exposition has ever been carried through to support this assertion, so often met, and so widely accepted. The conditioned reflex is a highly simplified picture of animal behavior, simple enough to be readily comprehended and to attract converts in hosts, but too simple to be an adequate explanation of a complicated type of event. Perhaps if Watson possessed a more adequate learning-formula he might be able to speak with more certitude, and offer definite case studies in support of his assertions. Humphrey (10) ten years ago attempted to explain a number of psychoanalytic terms as aspects of conditioning, arguing that projection, rationalization, and other mechanisms are attributable to the simultaneity of stimuli on a past occasion. For example, he considers the case of a girl who displays unusual solicitude for the mother. The psychoanalyst explains her fondness as a reinforced effort on the part of the Conscious to suppress an Unconscious wish that the mother die. This is the mechanism of *compensation* (not to be confused with the other uses of the same word in the Adlerian literature). Humphrey believes that we have here a case of a stimulus conditioned to a new response:

> "All that we can see is that by the principle of the conditioned reflex a stimulus to one of the opposing tendencies produces emotions that are due to the other, because the two courses of action have been experienced near together in time."

Conversely, one might infer, if a girl loved her mother wholeheartedly, or in psychoanalytic language, with both her Uncon-

scious and her Conscious, and the idea of her mother's death was suggested to her during her tender moments, that thereafter every stimulus to display affection to the mother might evoke the thought of murder. Other statements follow:

> "The emotional 'unconscious' complex dealt with by the Freudians is a very particular case, and it is quite inadmissible to argue therefrom as to the general nature and effects of complexes."
>
> "Unconscious wishes there may be; but of the synthesis of the wishes into a system in normal cases there is no evidence."

In short, the theory attacked is not wholly rejected, the proposed system proves inadequate as a substitute, and the position of the writer becomes a sort of inconsistent eclecticism. The fault lies in the limitations of the conditioned reflex as an account of the learning-sequence, and also in the fact that a bare scheme of the process of learning will not suffice, unless the laws which operate in the process also enter into the explanation.

Significant is the fact that in discussing *transference* Humphrey is forced to abandon the conditioned-reflex account and rely upon a somewhat more complex formula:

> "It sometimes happens that one detail will play the part of a psychological Atlas, bearing upon its shoulders the full reaction for the whole system. This, it should be noticed, often leads to pathological results" . . . "It does sometimes happen that an insignificant stimulus will of itself produce the whole reaction. The conditions and the explanation of this phenomenon are obscure, but the facts are unmistakable."

AN ADEQUATE SYSTEM

In recent years our idea of the essential learning or conditioning sequence has undergone modification—modification superficially slight, but indispensable to systematic perfection. I am inclined to accept Hollingworth's scheme, which he calls the *Theory of Redintegration* (7), as an adequate and completely developed outline of the learning sequence. Taking it in combination with the Law of Effect, we can certainly demonstrate an adequate basis for the task of rendering psychopathology systematic.[1]

Since the redintegrative scheme is by now familiar, it will not be necessary to describe it in detail. Briefly, its two essential features are the combination of stimulus-details, and the ability of stimulus-details to function for their past contexts.

[1] The writer must acknowledge his indebtedness to Dr. Hollingworth for both teaching and inspiration. Dr. Hollingworth read the original draft of this paper, and offered several helpful suggestions.

(1) Stimuli function not independently, but in groups or situations. A stimulus-situation may be composed of any number of stimulus-elements, each calling forth a response which may involve a few muscles or the entire set of effectors. The final response will be a sort of algebraic summation of partial-responses—a sort of composite photo. If one stimulus evokes the raising of all the members, and a second evokes only the lowering of the right hand and the raising of the right foot, then the right foot will be raised higher than by the first stimulus alone, while the right hand may be raised to a lesser extent, or possibly lowered.

(2) *If the consequences of the responses are proper,*[2] redintegrative learning takes place. It is not that one stimulus without potency, through mere association in time, takes over the response of another. Rather, a detail in a stimulus-situation acquires the potency by itself to evoke the entire reaction which its entire situation once evoked.

That process which Humphrey considered obscure and exceptional is now recognized as the invariable mode of learned reactions. The conditioned reflex is but a crude variation of it. If on one occasion a response (of the entire body) follows a complex stimulus (a complete pattern of sensory stimulations) then any fragment of that stimulus may on a later occasion call forth the entire response. If once a boy was frightened by a dog in a narrow, dark corridor, with a closed door, then any dog, a narrow place, darkness, or any closed door may arouse fear in him. If a dog pricks up his ears, salivates, turns his head, and shows other reactions—on hearing a bell, smelling meat, seeing a room, and undergoing other sensory stimulations—then merely hearing the bell will evoke the total reaction once given to the entire past situation.

Certain implications follow: The cues which evoke a response may be reduced to very fine details of the original stimulus; situations only similar will evoke the same responses; if a present stimulus has more or less elements in common with a past stimulus, it may evoke the same response.

Although this formula is adequate as a schematic outline, the actual event in nature is inconceivably complex. Reactions occur to configurations of stimuli and to partial details of these configurations in new contexts.

Pleasure and the Law of Effect—Hollingworth (6) has pointed out that pleasure is not a force stamping in a response, but merely

[2] This stress on the after-effects of a response is not really typical of Dr. Hollingworth, who stresses the bare outline of the redintegrative sequence.

a sign that stimulation is being terminated. Annoyance is likewise the mental experience accompanying the state of being stimulated. The law of effect should be stated as follows, that stimuli, or irritants, become attached to those responses which eliminate them. While pleasure may accompany any response which eliminates a stimulus, it is the eliminating effect of the response, not the accompanying feeling-state, which causes learning.

The most important implication of this valuable insight is not made clear. It is necessary to convert it into neurological terms, as follows:

When a response terminates a stimulus, the nervous receptor-to-effector pathways being traversed at the time tend to remain open, or easily traversible, and will be traversed the next time the same receptor cells are stimulated.

In this form, the Law of Effect may well appear to be the one essential determiner of redintegration. The classical objection against the Law is removed—the objection that the pleasure-force cannot work retroactively upon a response already completed.

Let us here define effective responses as those which terminate stimulation and bring pleasure.

Potency and Dominance—It has been charged that these terms are acquiring wide usage without being clearly understood. The fact is that the concepts are limited only by our ignorance of certain details of the learning process. Dominance is the property of that stimulus-detail which effects its own response in competition with the other elements in its stimulus-context. Dominance may result from the greater potency of the stimulus-detail or from reinforcement by other details whose responses are similar. Potency is the likelihood which a stimulus has of being dominant in a given context. The more potent a stimulus, the greater the annoyance which it will arouse if not readily eliminated.

Trial and Error—The actual change in responses which is a feature of learning occurs during the trial-and-error sequence. When a response fails to eliminate stimulation, then the stimuli which are dominant, and which are evoking that response, lose potency for the moment. Dominance is acquired by another detail in the context, which in turn loses dominance to a third if its own response is not effective, and so forth.[3]

[3] The trial-and-error sequence and short-circuiting have been ably discussed by Hull (9). For an account of the origin of specific responses to various stimulus-details—their emergence out of random behavior—see Holt (8, Chaps. 6 and 7).

Thus if hunger is stimulating us and in response we have entered a kitchen and are tugging at an icebox door that refuses to open, our annoyance mounts, and suddenly we lose interest in the icebox and are attracted by a cupboard. If we open the cupboard and our response turns out to be ineffective because we find no food, then some other object will next catch our eye. We must imagine that a continued transit across a nervous pathway soon results in blocking that pathway, with the result that other nervous transits are left to arouse their effectors without competition. *When a stimulus is terminated,* the efferent pathway functioning at the moment is the one which remains traversible, and all afferent series which are discharging at the time will tend to discharge into it when they are next stimulated, rather than into their original efferent pathways.

How frequency functions is in doubt. Probably, each effective occurrence of a response increases the existing number of its stimulus-elements, since on each occasion the stimulus occurs in a *new context,* and each detail therein becomes attached to the effective response. Thus after each conditioning there is a greater likelihood that any stimulus leading to that response will find reinforcement in its contextual situation. Also, under certain conditions the potency of a detail in itself may be increased. Frequency stamps in a response only when the conditions of the Law of Effect are met.

It would be arrogance to assert that the above account is a universal one, or even that there are many who would readily accept it in its entirety. We can point out that its constituent parts are similar to those found in other orthodox accounts, and that we are not so much submitting *new* laws, as we are discarding a certain number of familiar formulas and *selecting* those which stand the test of scientific evaluation. We might assume, for example, that frequency alone can stamp in responses, or, as has been assumed, that new responses to be "stamped in" come from nowhere to begin with; and such assumptions would render our later explanation of abnormal behavior much simpler. We might assume that ineffective responses are directly stamped out by the accompanying annoyance, instead of being gradually "short-circuited" out, or make that common but mistaken assumption that stimulus-response bonds which are not constantly evoked and reconditioned must necessarily die out while in a passive state.

We refrain from employing an eclectic, or rather miscellaneous system of laws, and confine ourselves to a theory which seems entirely consistent with nature. On the faith that we have a com-

prehensive and reasonably accurate theory of learning rests the hope that we can now explain the phenomena of abnormality.

The emotional state—Any mental event has its affective element of pleasure or annoyance. An emotional state involves an intense affect, being the response to a particularly potent stimulus. Emotional states are differentiated by different degrees of affective intensity, and by the presence of different sensory qualities from organic and proprioceptive sources. Dunlap (2) has pointed out, however, that what differentiates one class of emotions from another class, as *fear* from *anger,* is not any peculiar pattern of intra-organic sensations, but simply the nature of the cause—whether the stimulus-situation is the classic bear, or the classic muttered insult. Affective elements are most likely sensations themselves, and it is profitable to assume that they are intra-organic sensations *whose source is nervous tissue.*

A person receiving potent stimulation, hence in an emotional state, is *distractible.* One stimulus after another rapidly loses dominance, and a rapid trial-and-error series is shown. Moreover, since he is assailed by potent stimulation, he shows no response to minor cues in the environment. Now *sagacity,* or scope, or intelligence, is the ability to respond sensitively to a large number of cues in the situation, to show the effect of subtler stimulus-elements in one's reaction. A person subject to a potent stimulus is therefore typically *unsagacious.* He is of course restless and tense.

The Neurotic Personality—It follows quite simply that the neurotic constitution is one to which stimuli tend to be more potent. Irritability, or sensitivity, is the characteristic of the neurotic. This may be a function of neurochemistry, or of corticothalamic inbalance (5, p. 327), or of neither. There is no reason why there should not be significant individual differences in this trait, whatever its organic basis.

Without here discussing the classical neurotic entities, let us take the generalized concept of "nervous" or "emotional" instability. The unstable individual shows three typical characteristics: He is distractible, hyperemotional, and hyperactive. That is, he tends to receive potent stimuli.

His responses to situations are frequently unsagacious, as Hollingworth has pointed out (5, p. 250), whatever his sagacity may be when he is fairly passive.

The neurosis of an individual is a function of his environment as well as of his sensitivity. Given the proper environment, con-

ceivably any individual may show a normal picture. The neurotic tends to be potently stimulated, or in a state of annoyance, in situations where the normal individual feels only mildly. His stimulation is usually too sustained, and when effectively responded to, too soon renewed, and he is regularly in a state of annoyance. Only let such an individual acquire a response which gives slight pleasure each time he performs it, and he will give the reaction repeatedly. Thus he will show an eye tic or a shoulder tic in response to almost any potent situation. For he is distractible, and gives rapidly changing trial-and-error responses to all potent stimuli; almost every situation will include some stimulus to the tic, which will acquire dominance for a moment in the course of his reactions.

Tics are usually effective, since they relieve slight discomfort from eyestrain, clothing, or other irritants. Remove the pleasure of the eye tic by reducing the sensitivity of the conjunctiva (1) and the patient finds another dependable source of pleasure to form his 'nervous habit'.

He has a tendency toward intense emotional experiences, so intense that after a single occurrence the effective terminating response will be well implanted—many potent stimuli will be formed. For weeks and months (after a shock, for example) his environment will teem with potent stimulus-details which evoke the response. Years later one of these details may evoke it again, and if it happens to be reliably effective under the new circumstances, it may acquire many stimuli again, and continue for a lifetime.

Normal people under potent stimulation, as when self-conscious, are distractible, show rapid trial-and-error changes in response to situations, and in the course of their responses show tics, spasmodic movements, and hysterical paralysis of muscles. The neurotic receives potent stimulation from a normal situation.

Psychoanalysis discovers how in the past we acquired a certain reaction after an emotional experience, provided that emotional experience was connected with sex or a feeling of inferiority, and not with hunger or eyestrain. It also discovers certain persistent causes of potent stimulation in our present environment, and explains certain reactions by discovering their present stimuli, and inducing the patient to remember the conditioning episode that formed the bond. The analyst succeeds provided the facts meet his bias, he has good fortune with his patient's memory, and is able to judge competently the reliability of the reports he is given.

Motives—Orthodox psychology itself is not free from the assumption of enduring entities within the organism which cause persistent behavior. The *motive*, the *purpose*, and the *desire* are still in common use as causes of prolonged or repeated activity. To be sure, the academician will grant that without doubt each specific response has its specific antecedent, or stimulus, in nature, and that there is nothing more to behavior than the occurrence of stimulation in a receptor, the discharge of a nervous impulse, and the explosion of stored energy in muscles. He will grant that the desire or purpose is a transitory event, and that between its occurrences it has no existence. The layman's terms still cling, however, to his accounts of human behavior.

The present writer has undertaken to demonstrate (3) that all prolonged or recurrent behavior may be attributed to transitory stimuli, and that whatever of consistency or similarity there may be in certain behavior is due to the presence of fairly stable nervous traces. The demonstration of this proposition, and the analysis of various types of motives, is an indispensable preliminary to our subject, but since it has been covered in full elsewhere, and since a summary will not make the point, we shall not enter into it here.

Mental Events in a Causal Sequence—The account to follow will assume that matter is constituted of the same neutral substance as that of mental events, and that the mental event, or thought, is identical with some physical reaction of the body. The mental event is regarded as one of the causal chain [4] of physical events (13).

As for the objective phase of mental events, there is good reason to believe that they are effector patterns, and relatively little reason to suppose that they are cerebral patterns (11).

We have paused to present these two points of view, but should point out that they are not essential to our forthcoming interpretations. The discussion to follow is not incompatible with more conventional points of view.

THE TERMINOLOGY OF PSYCHOANALYSIS

The Unconscious—The Unconscious probably ranks first among the psychoanalytic concepts in popular appeal, being most colorful, obscure, and unenlightening. The Unconscious (or, instead, some component of it) is often an individual within an individual, who desires, struggles, thinks and even devises clever subterfuges

[4] For an analysis of deliberation and decision, see (3).

to work his will upon the Censor. Not only that, but he is identified with evil, he struggles only against civilized motives, and if he cannot break his prison and urge the individual to incest and assault, he likes to cause neurotic symptoms and slips of the tongue, and will sit patiently day by day operating a tic mechanism or an anesthesia for his host.

If we pursue a Freudian concept through the literature of psychoanalysis, we find that it often changes in form from paragraph to paragraph, and changes its name occasionally. Thus the Unconscious is vaguely identifiable with the Id. It can be used interchangeably with Libido, or other of its own contents or constituents. It is the location in space of suppressed complexes and the sum of these complexes. Unchaste thoughts are thrust into the Unconscious, the Unconscious creates unchaste thoughts and tries to force them into consciousness.

Behavior usually occurs without an experienced desire as an antecedent. Sometimes we may go through this typical trial-and-error sequence: Undergo stimulation of a receptor, experience a situation, experience several purposes or plans of action, act overtly. As this sequence is repeated, automatization occurs. The experience of the situation (perception) and the purposes drop out, and the only remaining reaction is the last, or skeletal one, which occurs at once to the stimulus.[5]

Where desire or purpose does not occur, it need not be assumed to be present as an unconscious desire (whatever they may be) *because a desire is not a necessary antecedent of action.* A potent cue may arouse its skeletal reaction without arousing the thoughts that once occurred with it. This is in fact the rule—most reactions are automatic.

Whatever the Unconscious may be in addition, it is primarily the dwelling-place of thoughts and desires which once occurred with certain responses, and which are no longer observable because those responses are now automatic. Presumably the wrong turns which rats make when they begin to learn a maze, go into the Unconscious when they drop out.

But we can explain automatization in physiological terms: All trial-and-error series undergone, including those known as desiring or planning, leave traces on the nervous system which may eventually enable the initiating stimulus to evoke the terminal act immediately.

The Unconscious must be discarded if a simpler hypothesis will do as well. We have seen that we can *explain* certain

[5] The analysis of this as a typical trial-and-error sequence is given in (3).

peculiarities of psychological sequences in *physiological* terms. A second point remains: Can we *formulate* the laws—the dependable temporal sequences and the dependable quantitative relationships—without assuming an Unconscious?

Now the observable data in all psychological sequences are: (1) the stimulus-situation; (2) the past history of the individual, the continuum of situations to which he has been subjected and has responded, for segments of which a single stimulus can function in the present; (3) the effector response.

We can formulate abstract laws of sequence, and even quasi-quantitative laws[6] in these terms, and have them applicable (parallel) to all observable psychological phenomena; for this proposition we are patiently accumulating evidence. If we use no further concepts, we can still have a theory thoroughly applicable to nature, because no other psychological phenomena are to be observed in nature.

As for the suggestive value of the Unconscious hypothesis, the assumption of a substance "consciousness" which can be attached to or detached from thoughts, and the conception of thoughts as states or objects rather than events, these points belong rather with the treatment of the *Fore-conscious* or *Pre-conscious.*

Let us overlook the incidental inconsistency, the superfluity, and the obscurity of the concept. We see two specific properties characterizing the Unconscious; the assumption of both is unwarranted. First, the Unconscious is considered a *constant* entity, on the grounds that motivation continues between reactions. A continuous cause is thought necessary for occasional events! But all animal activity can be explained in terms of present transient stimulus and immediate consequent. Second, the Unconscious is an internal mechanism which *makes and ejects whole reactions outright;* but all reactions are caused by the physical stimulation of an internal or external sense receptor, plus the redintegrating action of the nervous system.

The Unconscious is in fact definable as "that unknown thing, whatever it is, that causes behavior otherwise unexplainable."

"What makes that man jerk his head so, Father Freud?"

"It is merely because he has an Impulse-to-jerk-his-head. Whenever we have an impulse to do a thing, we do it."

"Well, then, tell me about this Impulse. What is it like?"

"It is a part of the Unconscious, my son."

The Id—The Id is an entity suggesting more dynamic prop-

[6] Examples are the sub-laws of redintegration (see 7) and the relationship between potency of stimulus and intensity of affective experience.

erties. It stores the libido and also habit tendencies, tries to express instinctive urges, and is utterly impulsive and irresponsible. The Id is understood either as another mind (when it is not considered a mere receptacle for various entities) or as a sort of other individual or demon within us. Though we try to strip it of its fantastic properties, we observe that its activities are often human. Striving to manipulate thoughts and acts, it struggles and wishes. It is by nature malignant or troublesome.

This is a working hypothesis to be sure, but it is the most primitive and naïve type of assumption in the history of civilization. The early savage explains natural events as caused by anthropomorphic spirits, and since events are favorable or unfavorable, he believes in good or bad spirits. Spirits dwell in trees and rocks, spirits make the tides flow and the sun rise. And in the twentieth century, they cause neuroses, forgetting, typographical errors, wit, and humor!

Possibly we exaggerate. Possibly we are speaking only for those who understand the theory amateurishly. We may be carrying to its absurd conclusions a mere attempt at explanation, a metaphor used to make simple and vivid the concept, and not to be taken literally. The Id is hailed by serious commentators as a brilliant contribution, although, as they state, it cannot be well distinguished from the Unconscious, and is not entirely clear to the foremost authorities (4, p. 35, 37). The test of value is apparently wide usage, or stimulation to the production of literature.

In all fairness, we should examine some of the descriptive remarks made about the Id or Unconscious, before pronouncing them obscure: The processes of the Unconscious are "not ordered temporarily, are not altered by the passage of time, in fact bear no relation to time at all" (4, p. 26). The Eros and death instincts struggle within the Id (p. 36). The Unconscious consists of mental life which has been repressed (p. 24). It contains impulses as well as ideas (p. 26). The repressed becomes a part of the Id (p. 36). The Id contains the libido, instincts, habit tendencies, repressed matter, and all phylogenetic acquisitions (p. 36).

The above are based on a *systematization* of the literature, a scholarly and thorough selection and classification.

Let the reader derive what enlightenment he can.

Complex—A stimulus may arouse a total motor reaction by functioning for the entire situation of which it was a part. But only rarely does it evoke a memory of its former context as part

of the reaction. A child will flee from a dog without remembering when it learned to fear dogs. It flees merely because it seems a dog, and not because of a reinstated memory. We use words without remembering each time the occasion of learning their meaning. When a fragmentary stimulus does restore its context to memory, it is still the stimulus, and not the memory, which is the cause of subsequent behavior.

Take now a case of "Œdipus Complex". A young man, suffering from an "unconscious desire" for his mother, finally marries a girl who resembles her in feature or mannerisms. The complex, "a group of ideational elements belonging together, invested with affects", which is repressed and which strives for expression in various ways, is said to be motivating his action when it occurs. If a complex is always present, why does it not manifest itself continually? Obviously it requires transient, occasional stimuli to arose a reaction. The story might as well be stated in this way:

To the mother as stimulus-object, there is a habitual sexual reaction, which has been finally inhibited. (Inhibition will be described later.) The reaction shows the redintegrative sequence in that it occurs to a similar object, the girl. A partial cue, that feature or mannerism in which the mother is resembled, functions for its entire former context.

This is an instance of transient stimulus and its consequent; no enduring mental entity need be assumed. The one persisting condition connected with the phenomenon is that physical change in the nervous tissue which was involved in learning the habit, and which now permits a redintegrative sequence to occur.

A case of abnormality—The response to the girl, in the case we are describing, may be "unsagacious"—the individual may react to the partial detail entirely in terms of its past context, without regard to present differentia; his reaction may be the inhibitory one instead of the first sexual one, and he shows impotence. Or he may respond "sagaciously", in terms of present as well as past; his response may be the joint effect of all elements in the present situation; the sexual reaction is as to the mother, but since the social relations are different in this situation, the socialized response of inhibition does not occur, and the mating is a healthy one.

Note that distastefulness does not place a topic within the peculiar province of psychoanalysis, and outside the range of laboratory psychology and its laws of learning.

Inferiority complex—The factors involved in learning any habit are well illustrated in the acquisition of an 'inferiority complex'.

That much human activity seems to be caused by a desire to dominate; that it is unpleasant to feel inferior, and pleasant to know that one is superior to other men; that individuals with marked defects of physique or of personality, or with inferiority feelings caused by some biographical incident, tend to show a disproportionate zeal in demonstrating their superiority in those fields where they excel; these are facts of observation.

An individual today plugs at his work and labors to distinguish himself in intellectual fields. We know that as a child he was inferior physically, and was made intensely unhappy by reminders of the fact. Shall we say that each ambitious move today is prompted by a suppressed complex?

The rat in the maze feels displeasure, in his case that of hunger, and shows a series of reactions. Whatever reaction finally eliminates his hunger is learned, no matter how absurd it may be; while ineffective reactions drop out. If the rat finds food after standing on his head, he will stand on his head when details of the same situation recur—the feel of hunger, or the sight of the maze, or the smell of the experimenter. Similarly, the boy feels displeasure, in his case that of disappointment, makes a number of ineffective reactions, and finds that displeasure is eliminated by the thought of intellectual superiority, by excelling, eliciting praise or admiration. Why this reaction should prove effective we need not know, nor need we know why the realization of relative inferiority is painful; (probably it is a matter of conditioning to fear).

The compensatory reaction proves effective *many times* during childhood, sometimes *eliminating violent states of displeasure.* After much learning it occurs to slight cues. The reaction was evoked and was effective *in many different situations* and there are many various cues which are potent now. Any suggestion of inferiority arouses the thought of distinction, or spurs him to work harder. Also, entering his study or town library is adequate stimulus to inspired plugging. Entering any library may be sufficient stimulus, and finishing a meal is stimulus to heading for a library, and subsequent grinding. His compensatory reactions occurred very frequently in classroom situations. Now he exerts himself in a classroom, at the sight of a book, on speaking to a student or teacher, when an academic subject is discussed, even

wants to memorize and take notes on church sermons. He wants to raise his hand and spout information at a public lecture.

As an adult, his old maladjustment and worries never recur to him, and if he were reminded that he once fretted over his physical disabilities, he might enjoy the story. His former disability may be gone; but many reactions still manifest themselves; the compensatory reaction still brings him pleasure for other reasons. He still strives to elicit praise. Necessarily, there are always traits in which he is inferior to some people; society stresses competition, and suggests that personal triumph brings pleasure; or perhaps he feels that a good reputation will ensure economic security. He retains the reaction but does not live over old experiences each time he shows it. The potent cues are numerous and varied; hence the reaction or reactions are frequent.

But they are not continuous. A trait, a habit, a neurotic symptom, occur in specific situations.

The individual is not always ambitious, industrious, or aggressive. For example: On Sundays he never thinks of working. When guests visit him, he strives to be hospitable rather than aggressive. He unbends with children. Before his wife and certain friends he is humble and modest. Certainly he takes time off from his work to eat and sleep! Careful studies have shown consistently that character traits such as honesty, coöperativeness and suggestibility are specific,[7] and are shown in certain situations and not in others, depending on the nature of the situation and the habits or reaction which the individual has acquired to that specific situation. Individuals do not show the "trait" of honesty or dishonesty, but are victims in each case of present situation and past biography. We do not assume an "honesty complex".

Yet, if the motivation for his ambition comes from a constant force, located in his bosom or his viscera, which is sufficient cause for his actions, *the trait should be constantly shown.* Again we see a psychoanalytic concept dissolve into an unwarranted assumption to explain specific instances of stimulus-response sequences. Note again: The one *continuous* condition explaining all these events is a physical one—the nervous-tissue traces that cause the relation between stimulus-detail and response.

In the case we are discussing, many of the apparently "compensatory" reactions may have no reference to childhood events or to inferiority. We may have learned our number concepts from

[7] Referring to the work of Hartshorne and May on deceit and on service, and to that of Brown on suggestibility.

handling matchsticks but we do not think of matchsticks when we operate in calculus. We have gone on to acquire new responses, relevant only to advanced situations. Now let us take our subject. Certain reactions first made in childhood, for self-display, are now used effectively for achieving economic security. But these reactions have been very much modified, and involve different objects, different people, places, aims, and behavior, despite an essential similarity. For instance, as a student he burnt the midnight oil, and now takes correspondence home from the office to work on. Shall we tie up his present behavior, new elements and all, with a feeling of inferiority, or with its actual present cause, the desire for bread-and-butter? He is neurotic. The trait that made him particularly sensitive to inferiority now makes him particularly anxious for economic security.

A neurotic symptom—Shyness, aggressiveness, smoking cigarettes, tics, insomnia and crime—a hundred traits, habits or symptoms have been attributed to the inferiority complex. And yet any given patient will manifest but one or two (hardly more) symptoms at a time. We might merely say, then, that our subject has acquired one or two habits which are socially unfavorable. If patients actually harbored "inferiority complexes", they should show all the various effects attributed to such a complex.

Substitute a compulsive act for the socially-adequate compensatory habit of industry which we attributed to our subject—say the subject bites his nails—and we see the explanation of a neurotic motor act, which is supposed to express symbolically a suppressed complex. Its first occasion is not remembered by the subject, hence is called "repressed". It occurs now without previous deliberation, hence is considered "dissociated from the main system". If the individual inhibits it because it is absurd or embarrasing, he feels displeasure, because some stimulus is evoking activity, and its consequent discomfort will not be eliminated until some effective reaction occurs.

Here, for lack of space, we must close with an enumeration of the topics which remain to be treated. We have covered what are probably the most essential terms, and the reader may be able to infer the method of disposal of others, particularly those referring to "mechanisms".

These and other relevant topics remain to be treated in later papers:

Repression, conflict, sublimation, the pre-conscious, inhibition, dreaming, symbolization, phobias, anxieties, sensory and motor

disturbances, compulsive acts and other neurotic symptoms, the pleasure and beyond-the-pleasure principles, war neuroses, dissociation, regression, fixation, rationalization, wish-fulfillment, slips and errors in everyday activity, projection, the trauma, free-floating affect and transference, the libido, visceral tensions, paranoia, introversion and extraversion, the super-ego, somnambulisms, ambivalence, the nature of the symptom and its conditions, sample case studies, and the psychoanalytic and other therapeutics.

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POSSIBLE DETRIMENTAL EFFECTS OF EXPERIMENTAL HYPNOSIS

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SINCE the time of Mesmer, there has been a general attitude of prejudice against, and fear of, hypnosis. Mesmer's own attitude did much to clothe the phenomenon in mystery and occultism and to awaken strong criticism against his discovery. The absurd claims made and the fantastic explanations given contributed to the development of scientific antagonism to the art. Further, the later utilization of hypnotism by charlatans made every honest man extremely wary lest even his sincere interest in it bring him into disrepute.

Fortunately, however, not all true scientists were frightened by the bugbear of public misunderstanding, and such men as Charcot, Liebault, Heidenhein, Janet, James, Hall, Prince, and Sidis have contributed greatly to a present rational attitude on the subject. As a result, there is a growing realization among the general public, both lay and professional, that it is a normal phenomenon of the human mind, fairly explicable, as are all other psychological processes, in our crude concepts of mental mechanisms. Further, there is a rapidly increasing realization of its great utility as a means of experimental scientific investigation, as well as a somewhat more slow development of its usefulness in the therapeutic field. In the latter regard, there is still a tendency to expect extravagant therapeutic results and, if they are not forthcoming, to discard the method as of little or no value. As is obvious, such utilization is wrong, since hypnosis, like every other psychotherapeutic procedure, should be looked upon as a means of approach to the problem and not as the royal road to the achievement of miracles.

As yet, present limitations upon the knowledge and understanding of hypnotism itself suggest that its greatest value lies in the field of psychological investigation and experimentation. Its use in psychotherapy is essentially empirical. No really rational use of it can be made until psychologists and specially trained investigators capable of plumbing the depths of ignorance

surrounding it and properly evaluating the psychological principles upon which it is based have explored it as a problem complete in itself. As a fertile field for investigation, its intrinsic value cannot be questioned. Whether or not the eventual findings will be of immediate and positive profit is entirely speculative and quite beside the question, since every realm of human activity and behavior is worthy of scientific examination.

This observation then gives rise to the question at what cost such a study could be made. If only at the expense of labor and study, well enough, but if hypnosis wreaks irreparable harm upon innocent subjects, it is time to call a halt and to devise other means of psychic exploration. The pathway of knowledge is already sufficiently strewn with costly human errors and it is always well to consider avoidance of more, however innocently and unintentionally they may be made.

A survey of the critical literature suggests that hypnotism is fraught with dangers, if mere opinions and ignorance can be given weight. But perusal of the various literary damnations of the phenomenon awakens serious distrust of their cogency. In one bitter attack on hypnotism published about 1850 (the identification of the reference has been lost) the author naïvely declares, "I thank God I have neither witnessed nor practiced the abominable art", following which, in dogmatic statements, he attributed innumerable evils to "The New Witchcraft". Then, in more recent times, the practice of hypnotism has been considered synonymous with the enslavement of the personality, the destruction of the will power, and the automatizing of the innocent subject. The attacks consist of personal opinions founded on abstract concepts no longer tenable, and supported by hearsay evidence. In brief, the literature is barren of anything deserving of much scientific credence. Even as late as 1924, a well-known journal of forensic medicine contained a long editorial emphatically denouncing hypnosis, adducing reasons therefor from innumerable opinions of lay people and considerable hearsay evidence. No references of scientific value were offered except the editor's own subjective opinion based upon some experience and founded upon the concepts of faculty psychology extant 40 years ago. This same editor now has a book in press on forensic medicine, that, he assures the writer, embodies the same views, many of which are the same as those first advanced against Mesmer when the term "Black Magic" was supposed to classify hypnotism adequately.

Other than these unfounded denunciations, the literature contains only sincere speculations upon possible detrimental effects, some of which are partially founded on actual evidence and experience but none of which are dogmatic, nor, for that matter, carefully controlled and evaluated. Even the literature on experimental hypnosis is most unsatisfactory, since practically none of the experimental work published has been carefully planned and controlled. Findings are empirical in nature and are indicative only of possibilities. The same holds true in the psychotherapeutic aspects, with the result that the whole field is considerably uncultivated in character. Accordingly, there can be no room for any dogmatic declarations, except the need of caution and of honest observation.

The question of possible harmful effects from hypnosis is one worthy of serious consideration, fully as much so as the possibility of beneficial results. Fortunately, it is no longer necessary to consider such things as "the emanation of a secret power", "the control of a weak will by an overpoweringly strong will", "the irresistible transmission of thoughts", and similar exploded superstitions. In the light of present-day knowledge, hypnotism, in intelligent circles, is looked upon as a normal though unusual and little understood phenomenon of the human mind, dependent wholly upon the coöperation of the subject, and which can be practiced by anybody willing to learn the psychological principles and technique involved.

But there are certain theoretical possibilities of harm which should be considered and studied, even though in the writer's experience findings in these regards are negative. It is true that these possibilities are entirely speculative and problematical, but a reasonable answer must be given before there can be a ready acceptance of this method of scientific investigation.

The first of these theories of possible detrimental effects centers around the question of the development of hypersuggestibility. The literature is barren of information in this regard. However, there is carefully planned and controlled work under way in a well-known psychological laboratory, and results so far, though not yet complete, are negative. In the writer's own experience, upon which it unfortunately will be necessary to a large extent to base the elaboration of these various questions, hypersuggestibility was not noticed, although the list of individual subjects totals approximately three hundred and the number of trances several thousand. Further, a considerable

number were hypnotized from three hundred to five hundred times each over a period of years. Also, several of the subjects were immediate relatives with consequent intimate daily contact and they were trained to respond, in experimentation, quickly and readily to the slightest suggestion. Far from making them hypersuggestible, it was found necessary to deal very gingerly with them to keep from losing their coöperation and it was often felt that they developed a compensatory negativism toward the hypnotist to offset any increased suggestibility. Subjects trained to go into a deep trance instantly at the snap of a finger would successfully resist when unwilling or more interested in other projects. Even when persuaded to give their consent against their original wishes, the induction of a trance was impossible. Nor were those subjects more suggestible to other people, since, when their services were "loaned" to the author's colleagues, the production of hypnosis in them, despite their extensive training, was just as hard as it had been originally for the author. And the same thing was found true when the author "borrowed" subjects. In brief, it seems probable that if there is a development of increased suggestibility, it is negligible in extent.

A second question is that concerning the possibility of the alteration of personality. Just exactly what such a question means is difficult to define, but at all events, the general significance of the question is comprehensible. As we all know, alterations of personality occur in response to suggestion in ordinary daily life. Hence it is only logical to presume that a state of enhanced suggestibility such as an hypnotic trance would show an increased susceptibility to alteration of personality. In the writer's experience, where members of the family were very frequently hypnotized over a period of four years, no alteration in personality was noted in any way attributable to hypnosis. The same thing was observed in the case of friends who were utilized as subjects over a similar length of time, and likewise with subjects met in the laboratory. But an even more forceful and trenchant answer may be derived from the experience of psychotherapists who have deliberately and carefully utilized the method to induce desired alterations in the personality of their patients with disappointing results and usually failed even when such was the goal of their efforts. The inefficiency of hypnosis in the treatment of homosexualism is an excellent illustration of the difficulties of fundamentally altering a personality. Briefly then, it seems that the conclusion may be drawn justifiably that experimental hypnosis

will not cause any fundamental alteration of personality or possibly even any alteration in addition to that which would accrue from ordinary personal contact. However, before this question can be left, there arises another concerning the charge that various dissociated personalities were created by the overzealous investigations of the hypnotist. Obviously, there can be no dogmatic answer to such a charge since the whole thing itself is a matter of speculation. That it is a possibility is to be admitted readily, but personal experience suggests that just as readily may it be characterized as an improbability. The one conclusion to be drawn is that the operator should be clear of vision and mind and of unimpeachable integrity, qualifications not to be limited to any one field of research.

During the induction of a trance the subject's contact with reality is greatly reduced. This gives rise to the question of the possibility of weakening the subject's perceptual powers concerning reality and unreality. An eminent practitioner in the field of behavior disorders reports that he utilized hypnotism as a therapeutic agent over a brief period of years, following his cases carefully, and discontinuing it because of a subjective feeling that they had lost somewhat the ability to distinguish between actuality and phantasy. But he added very honestly that this feeling was entirely subjective and that he could not offer any real evidence. When it is considered that his cases were behavior problems and presumably of unstable personality, and that the hypnosis was used for therapeutic purposes, it seems reasonable that such might have occurred. But before any great weight could be placed upon his subjective impressions there would have to be a careful evaluation of the many significant factors concerned. At best it is only a subjective opinion and serves only to contradict the writer's, which is entirely to the contrary but based more on experimental studies than on therapeutic work.

Also, in this same regard may be mentioned the religion of the "Christian Scientists" which is based fundamentally on the principles of self-hypnosis, and of which the entire philosophy of life centers around a denial of the difference between reality and unreality. However, even the most ardent, despite the honesty and sincerity of their wishes and practice, seem to have no difficulty in this regard, as the material prosperity of the church and its members attests. If an entire philosophy of life with training from infancy upward is insufficient to accomplish this, it is difficult to believe that a time-limited hypnotic trance for other purposes would accomplish such an end. From a purely psychological

point of view, the stimuli emanating from reality and those from memory traces within the brain are fundamentally different in their components, and it smacks of the miraculous to assume that a time-limited procedure could establish a fundamental alteration of the psychological habits established in a lifetime.

Fourth and last of the questions to be discussed in this paper is the probability of subjects acquiring unhealthy escape mechanisms as a result of their experiences. This speculation arises from the observation of a trick one of the author's subjects developed. He was a university student, "majoring" in psychology and "minoring" in art, and intensely interested in hypnotism. When given an art assignment of an original picture, having been taught how, he would take a crystal and crystal gaze until he saw a suitable picture which he would then copy. Or if it were an English theme assignment he would crystal gaze until he saw a typewritten outline of a suitable plot and then would fill in the necessary details. The question arises, could he make use of a similar type of mental mechanism to force himself to believe that some unpleasant duty was done or to escape some unpleasant situation? Or could the subject taught to show hypnotic paralysis or anesthesia later develop the same things as hysterical symptoms? However, the history of psychopathology is replete with evidence to show that the human mind, however lacking it may be in fundamental endowments, needs little instruction in devising complex escape mechanisms. Hence, it is much more reasonable though less disturbing to suppose that the added knowledge the subject derives from his hypnotic experiences gives him an increased understanding of himself. In the author's experience there was noticed only a tendency to utilize hypnotic experiences for profit, a finding substantiated by inquiry into the experience of other trained workers. Briefly, what little evidence there is and an attempt at a fair evaluation of theoretical possibilities suggests small likelihood of detrimental effects in this regard.

In summary then, the literature offers little credible information concerning possible detrimental effects of experimental hypnosis, although replete with dogmatic and opinionated denunciations founded on outworn and untenable concepts of the phenomenon.

Theoretical possibilities of detrimental effects are possible development of hypersuggestibility, the alteration of personality, weakening of the subject's perceptual powers in regard to reality and unreality, and lastly, the development of unhealthy mental attitudes and escape mechanisms.

The literature is barren of controlled experimental investigation of these problems. The author's own experience, based upon several thousand trances on approximately three hundred individual subjects, some of whom were hypnotized at least five hundred times each over a period of four to six years, reveals no evidence of such harmful effects. This clinical finding is further substantiated by the well-known difficulties encountered in the deliberate therapeutic attempts to occasion desired changes in the personality. Accordingly, marked changes from experimental hypnosis appear questionable.

EXPRESSION OF THE EMOTIONS IN A BLIND-DEAF CHILD

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BECAUSE of their complete isolation from ordinary forms of social intercourse, subjects who have been blind and deaf from birth afford unique opportunities for psychological study. While the behavior of such cases is unquestionably affected by the results of their own experience, this experience does not include observation and imitation of the behavior of others, nor, previous to training in language, can any except the most primitive and essential standards of behavior be communicated to them. In many respects, therefore, the behavior of such children may be regarded as the nearest approach to "native" reaction-patterns, freed from the influence of any social milieu, which can be observed in a civilized state of society.

Some time ago I had the opportunity of observing a ten-year-old girl who had been completely deaf and blind from birth. Up to the time of observation she had had no formal and very little informal training. Her parents are farmers in meager financial circumstances. The mother is obliged to devote practically all the time which she can spare from her housework to assisting on the farm, and has therefore made little attempt to teach M—— even the simplest matters of self-care.[1] Although the mother stated that the child was able to indicate her need for food, drink, or toilet by means of certain described signs, she admitted that these signs were not used very consistently. In our own experience we were not able to decide with certainty that the signs in question had any truly symbolic character. They were made under

[1] The question of the child's intellectual level has not been satisfactorily answered. During the first three months that she was under observation she was placed with an elderly woman who had had some nursing experience, but who, unfortunately, became so devoted to the child that it seemed impossible for her to adhere to any consistent program of training. She was then placed in the charge of a woman who had successfully trained one blind-deaf child (who, however, had had four years of normal sight and hearing before the onset of the defect). She remained with this woman for three months but showed very little evidence of improvement, and was therefore returned to her home as unpromising material for training. The cause of her defects is unknown, except that it is almost certainly non-venereal. The family history is normal as far as is known, and the three other children in the family have made better than average progress in school.

FIG. 1

FIG. 2

FIG. 3

FIG. 4

FIG. 5

FIG. 6

FIG. 7

FIG. 8

both appropriate and inappropriate conditions, and were frequently omitted when they should have been made. The child is gentle and docile in disposition and has never, so says the mother, required punishment of any kind. It would appear, therefore, that those aspects of her behavior which fall under the head of "expressive" movements may fairly be regarded as native patterns of reaction arising directly from the inner state of the organism, with little or no conscious direction toward modifying the behavior of others. The "language of gesture and facial expression" in such a child is almost purely egocentric. Social interaction plays a negligible part in determining its occurrence or pattern.

The child was observed[2] for an hour or more daily over a period of several weeks. During this time, careful notes on her behavior were made and several reels of motion pictures as well as a number of still photographs were taken. The accompanying figures are enlargements of single frames from a 16 mm. film. Figures 1–6 represent successive stages of behavior in a single episode which may be described briefly as follows:

A small china doll was dropped inside the neck of the child's dress.[3] Figure 1 shows her immediate reaction. The body, particularly the neck and shoulders, is tensed, the mouth is half-opened. The sightless eyes are opened to their fullest extent and the eyebrows are raised. The left hand at once begins to grope for the toy. Both the posture and the facial expression are suggestive of what we should ordinarily interpret as startled attention.

The toy fell between the child's dress and petticoat. In groping for it the hand was at first inserted under the petticoat, so that, although she was able to feel the outline of the toy she was unable to grasp and withdraw it. The hand was withdrawn and fresh effort made a number of times in succession, but each attempt was frustrated by folds in her clothing. Figure 2 shows her manipulating the toy through a layer of clothing. The bodily tension so apparent in Figure 1 has relaxed, as have also the eye-muscles. The head is turned slightly to one side, the lips are somewhat parted. The expression is one of interest and attention but without the element of startled surprise shown in Figure 1.

After several minutes of patient effort had yielded only failure the child ceased her groping and assumed the posture shown in

[2] Observations were made by the writer and Dr. Dorothea McCarthy. A note on the vocal sounds made by this child was published by Dr. McCarthy in *Ped. Sem.*, 1929, 36, 482–484.

[3] So far as is known, this was the child's first experience with a situation of this kind.

Figures 3. She did not really cry but made slight whimpering sounds, the head was dropped forward on her breast while the left hand picked at her nose—an unbeautiful but characteristic form of behavior when she was disappointed or displeased. She remained in this attitude for about two minutes.

Suddenly, as if struck by a new idea, she renewed the attack; this time from a different angle. Instead of reaching upward from underneath her clothing, she inserted her hand as far as it would go into the neck of her dress in an attempt to secure the toy from above. This time her behavior was far more violent and took on the appearance of a struggle, determined in part by exasperation and mild rage. Her body writhed and twisted; the right hand impatiently beat the arm of the chair. When at last she succeeded in grasping the head of the doll her difficulties were not ended for its feet had caught in her clothing so that only by a series of rather violent jerks and pulls could it be extricated. Figure 4 shows her in the midst of the struggle. The head is thrown back; the muscles of the neck and shoulders are visibly tensed; the feet are twisted about each other; and the retracted lips display the clenched teeth.

Figure 5 shows the instant of success. She has thrown herself back in her chair with feet drawn up under her. Both the hand containing the doll and the empty hand are raised in an attitude of delight, which is further attested by peals of hearty laughter. In Figure 6, taken an instant later, the doll is clasped lovingly in both hands as she beats its head against her teeth (a favorite method of securing vibratory sensations); while the exultant laughter shown in Figure 5 has faded to a smile of pleased satisfaction.

Figure 7 shows the child in one of her most characteristic attitudes and activities. She is seated on the floor with head thrown far back and lips parted. In her right hand she holds a table knife which she is beating with a spoon held in her left. The expression of absorbed interest in the vibrations thus produced is very marked.

To the writer, one of the most interesting forms of behavior shown by this child is her dance. She has never seen any one dance; she cannot know that others do so. Yet she dances; not in the simple hop, skip, and jump style of normal children but in an elaborate pattern of bows and whirls, bendings and posturings in which the head, arms, and entire body participate. Her dancing is usually performed on tiptoe as in the characteristic pose shown in Figure 8. In its more violent figures she sometimes drops to

the floor in a sitting posture, usually with legs crossed, where she remains for an instant or two, weaving her body from side to side, then rises and continues her dance without breaking its fantastic rhythm. The dance is clearly an expression of pleasure. It is usually accompanied by laughter and sometimes breaks up in wild peals of such violent laughter as to prevent her continuing. It may occur spontaneously when she is in good spirits, apparently as an expression of general *joie de vivre,* or it may sometimes be stimulated by giving her a bit of jelly or some other simple treat.

For the most part the child seems cheerful and docile. She laughs frequently and in response to appropriate stimuli. Her laughter is clear and musical, in no way distinguishable from that of a normal child. Her vocalizations are normally pitched and entirely free from the loud strident tones so often heard among the deaf. In spite of her usual docility, however, she has at times shown unmistakable signs of anger, which once or twice have amounted to real "temper tantrums". The occasion for these displays has usually been some kind of thwarting of her activities, particularly attempts to overcome certain unpleasant personal habits such as pushing and manipulating her eyeballs, an activity which, it may be inferred from her behavior, yields tactile sensations of particular interest to her. Mild forms of resentment are shown by turning away her head, pouting the lips, or frowning; sometimes by crouching down into a little heap with head on knees, or by thrusting the thumb and index finger into the nostrils as shown in Figure 3. More intense forms are shown by throwing back the head and shaking it from side to side, during which the lips are retracted, exposing the teeth which are sometimes clenched. This is accompanied by whimpering or whining noises, rising at intervals to short high-pitched staccato yelps. In her most violent outbursts the entire body is thrown back and forth; the feet are twisted around each other or beat violently upon the floor; the vocalizations are intensified and as a rule become shriller in pitch; and the head and chest are beaten with sharp flail-like movements of the arms. These blows are usually struck with the open hand, but at times the clenched fist is used.

As a rule the child's movements are slow and cautious. In an unfamiliar environment she hesitates to move unless one hand is in contact with the wall, from which she ventures out a little at a time with much circumspection. Even in a familiar room, she either moves very slowly, taking short steps, or keeps one hand extended before her as a means of avoiding obstacles. She dislikes to wear shoes which is quite understandable since she

apparently gains much of her knowledge of the environment through her feet. A linoleum of slightly different texture from that with which she was familiar was at once recognized as new, and for two or three days there was visible hesitation upon first stepping on it. It may be mentioned in passing that when seated in a low chair she frequently picks up objects from the floor with her feet instead of reaching for them with her hands. It is said that she has been known to pick up small objects with one foot while standing or walking, securing the object with the toes and passing it into the opposite hand. I have not personally observed this, although I have made repeated efforts to elicit the reaction by putting favorite small toys in her way.

Although her behavior suggests timidity, I have not seen any instances of active terror. The nearest resemblance to real fear was shown when the first attempts were made to teach her to go up and down stairs which was a completely new experience to her. Her immediate reaction when first confronted with the stairs was to hold up both feet and cling tightly to the experimenter, attempting to scramble into her arms.[4] She did not cry, but breathing and heart-beat were perceptibly accelerated. After the first few trials, however, although the resistance and clinging reactions continued, her general behavior took on a strong resemblance to that of normal children or adults during an exciting ride on a roller-coaster or shoot-the-chutes, where the slight feeling of insecurity or danger lends added zest to the experience. There would be occasional short peals of excited laughter, and more than once upon the completion of a trip she would turn toward the stairs in apparent anticipation of the next trial. It is interesting to note that her nearest approach to a symbolic or associative vocalization occurred in connection with this new and apparently highly exciting experience.[5]

During recent years it has become fashionable to minimize the importance of the "native" element in emotional behavior and to regard the "expression of the emotions" as in the main due to "learned" reactions, acquired as the result of social experience. Here, as in other studies of related factors, it is well to consider the sequence of the cart and the horse. In the psychological laboratory we have learned that even such simple reflexes as

[4] The scrambling reaction is not confined to the stair situation. The child is extremely fond of petting or caressing and a characteristic form of behavior upon coming in contact with any human being is for her to cling tightly to them and try to scramble into their arms.

[5] McCarthy, Dorothea. Notes on the Vocal Sounds of a Deaf-Blind Child. *Ped. Sem.*, 1929, 6, 482–484.

winking or the knee-jerk can be greatly modified through training and experience. Nevertheless, we continue to regard the reflex patterns shown previous to training as the "native" forms of behavior, while the modifications introduced by training constitute the "learned" elements. The evidence in this case is fairly clear, since in the ordinary course of life, little or nothing will occur to disturb the characteristic patterns of these reflexes. The modifying circumstances are artificial in the sense that they must as a rule be purposely introduced. Under a different set of environmental conditions of a kind which would naturally and inevitably bring about the modifications which we now induce only in the laboratory, separation of the learned from the unlearned elements would become much more difficult. It is this latter condition which we encounter in the ordinary studies of emotional expression as seen in normal children and adults. From the beginning, children find that certain forms of expressive behavior repay them better than others. Through observation and imitation of the behavior of others, and through experience of the reactions of others to their own behavior, the original or "native" reaction patterns become modified in various ways, but this does not mean that no such patterns existed prior to their modification.

In the emotional behavior of the little girl herein described, whose experience has been devoid of opportunities to observe the behavior of others except in the very limited and crude ways permitted by the cutaneous senses alone, from whom all except certain very gross forms of the reactions of others to her own behavior are likewise hidden, and with whom almost no conscious attempts at modification of emotional expression have been made, a strong resemblance to the well known descriptions of Darwin and Spencer may be seen. This suggests that the primary forms of expressive behavior are determined by native factors. Although among persons with normal sensory equipment, the original reaction patterns may become so overlaid with a veneer of socially accepted forms of behavior that it is difficult to distinguish the native from the acquired, nevertheless, the essential similarity of many of the reactions of this child to those seen among normal children makes one willing to hazard a guess that the veneer is not so complete nor so impenetrable as many have supposed.

A METHOD OF SECRETLY OBTAINING DUPLICATES OF WRITING

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IN order to obtain data in regard to honesty or truthfulness, or other facts concerning the behavior of a subject which he wishes to conceal, it is frequently necessary to resort to surreptitious means. One of these means is the method proposed by Voelker [1] which procures a secret impression copy of writing by placing a piece of paraffin paper underneath the page which is written upon. A pseudo-explanation is given in order to justify the presence of the waxed paper and its subsequent removal. Illegitimate changes and additions to the record are therefore evidenced by their absence from the waxed sheet.

An apparent difficulty with this method is that the subjects are frequently suspicious of the wax paper and therefore do not behave normally. It seems probable also that these suspicious individuals are the ones having greater insight, which fact may suggest that the frequently-observed correlation between intelligence and honesty, as measured by this method, is a spurious one. Another difficulty is that the impression on the wax paper is frequently very faint or absent altogether.

It is believed that the method, as described below, is an improvement in that the subterfuge is less apparent, and also that the procedure is not so involved or difficult.

The general method of procuring a secret impression copy is followed, except that, as a substitute for the waxed paper, a white and therefore invisible coating of powdered lead carbonate is applied to the back of the paper which is written upon. This paper should preferably be of the unsized sort generally used for mimeograph copies. The chemical resembles talcum powder and can be lightly dusted on the paper with a powder muff. It should be rubbed in with the palm of the hand and the residue shaken off. The coating is perfectly invisible when carefully applied.

[1] Voelker, Paul F., The Functions of Ideals in Social Education, Teachers College, Columbia University, 1921. Contributions to Education No. 112.

When the paper is written upon an invisible impression is transferred to a lower sheet. This impression resembles a carbon copy with the exception that it must be chemically treated to make it visible. This can be done by immersing the sheet in an atmosphere of hydrogen sulfid gas which turns the white powder black and brings out the impression just as plainly as if it were an actual carbon copy. Since there is no actual carbon sheet interposed, however, and because of the great adhesiveness of the powder, the combination is very sensitive and leaves a distinct copy of the very faintest writing.

A quicker and more effective method of developing the invisible copy is to immerse the sheet in a solution of ammonium polysulfid. The solution can be lightly sprayed on the paper with an atomizer if it is convenient to do so. A small amount of sugar may be added to the solution, which serves as sizing and keeps the copy from smudging.

The practical uses of the method as outlined above will probably suggest themselves and may vary a great deal in order to meet the demands of each particular situation. If, for instance, it is desired to adapt the procedure for the purpose of discovering cheating in an examination, the answers are written by the subject upon the face of the sheet which is coated upon the back. The impression is made upon a sheet which is placed underneath. This sheet may be a part of the upper sheet which is folded over and under, obviously for the purpose of concealing the correct answers which the subject is told are written upon the underturned face of it. The sheet must be unfolded of course when the student uses it as a reference in grading his own paper at the direction of the experimenter. If he dishonestly adds to or changes his paper while grading it that fact is conspicuous by the absence of the additions from the impression copy.

Another method involves clipping a second-sheet below the coated sheet. Its real purpose must be disguised by having the subject use it for some other apparent purpose. On it may be written, for instance, a part of the examination which, nominally, the experimenter wishes to correct himself. This sheet, which of course contains also the superimposed duplicate of the answers on the upper sheet, is collected after the answers are written, while the subjects are told to keep the other sheet and grade it by reference to answers dictated by the experimenter or written on a blackboard. Dishonesty is again indicated by additions or changes in the script.

RETROACTIVE INHIBITION AND WAKING SUGGESTION [1]

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INTRODUCTION

IN connection with some work on hypnosis using the retroactive inhibition phenomenon as a method,[2] the present experiment was done on waking suggestion. It was deemed advisable to run this control experiment in view of the fact that a number of investigators such as Professor Clark L. Hull suspect that little or nothing can be done by hypnotic suggestion that cannot be done by waking suggestion. This experiment, therefore, was conducted in an attempt to inhibit retroactive inhibition by waking suggestion.

APPARATUS, MATERIALS, AND SUBJECTS [3]

Lists of ten three-place numbers were exposed to the subject by means of the memory drum designed by McGeogh. The subjects were all Yale students from the College, and Graduate and Professional Schools.

PROCEDURE

On the first day, subjects were tested for suggestibility by means of Hull's postural suggestion technique.[4] In this preliminary examination the subjects were blinded by means of painted goggles. For all the later suggestions the subjects were instructed verbally to be relaxed and fixate on the experimenter's eyes whenever receiving suggestions. They were given typewritten instructions regarding the anticipation method of learning numbers to one correct repetition.[3]

During the practice period of from eight to eleven days, an attempt was made to train the subjects to be suggestible to the

[1] The writer is indebted to Professor E. S. Robinson under whose personal direction this experiment was performed and to Professor Clark L. Hull for advice and use of apparatus. It is included as one of the appendices to the writer's doctorate dissertation presented to the Faculty of Yale University, 1931.

[2] Mitchell, M. B. Retroactive Inhibition and Hypnosis. J. Gen. Psychol. Cin. press.

[3] For more detailed description see doctorate dissertation, Appendix A, by the writer, Yale University, 1931.

[4] Hull, Clark L. Quantitative Methods of Investigating Waking Suggestion. J. Abn. & Soc. Psychol., 1929, 24, 153–169.

experimenter. The muscular contractures ordinarily so easily induced in the light hypnotic trance were suggested with varying degrees of success. For instance, on the second day of practice, the subject was seated in a comfortable chair, told to relax, and to look the experimenter in the eyes. He was then told to clasp his hands together and as the experimenter counted to five his hands would become stuck together so he would not be able to pull them apart. The experimenter speaking in a smooth, monotonous voice with low tones resembling the chant of a Catholic priest made these suggestions while making light passes over the hands:

> One—Your muscles are contracting.
> Two—They are contracting harder.
> Three—Your fingers are stuck.
> Four—Your fingers are stuck harder.
> Five—You can't pull them apart. You can try but you can't pull them apart. The harder you try the tighter they become.

The reactions ranged from those of the subject who flipped them apart with a foolish grin, through the subjects who pulled them apart with a little effort claiming they did feel stuck, to the subject whose hands seemed to be seized with an iron grip with very pronounced vascular changes and could not be separated until the usual countersuggestion was given.

On succeeding practice days, similar suggestions were given to make the forearm stick to the arm of the chair, the arm rigid in a flexed position, the feet stick to the floor, or the legs stick in a crossed position.

In addition to the suggestions, two lists of numbers were learned each day to eliminate the greatest practice effects before the experiment began. To further counterbalance practice effects, the experiment proper was performed with the conditions in the ABBA order.

Eight sessions with each condition given four times were arbitrarily considered as one cycle. Six of the eleven subjects were run for three cycles while the remaining five subjects were run for one cycle.

Each session consisted of learning two lists of ten three-place numbers and relearning the first list with suggestions preceding each memorization. The suggestions given and repeated were as follows:

CONDITION A

A_1. *Given before learning first list.* You will learn this list but will forget it easily. By the time you have learned another list you

will not be able to remember this list. You will have completely forgotten it.

A_2. *Given before learning second list.* After you have learned this list you *will not* be able to remember the list you just learned. You will have completely forgotten it.

A_3. *Given before relearning first list.* Now you *will not* be able to remember the first list. You will only be able to remember the list you just learned.

CONDITION B

B_1. *Given before learning first list.* You will learn this list and will remember it very well. Even after learning another list, you *will* be able to remember this one.

B_2. *Given before learning second list.* After learning this list you *will* be able to remember the list you just learned.

B_3. *Given before relearning first list.* You *will* be able to remember the first list.

RESULTS

There was no reliable difference found for the two conditions in the mean number of trials or errors to learn either the first or second lists or to relearn the first. (Tables I and II.)

TABLE I

	Condition A	Condition B
O.L. List 1	11.11	11.29
List 2	11.30	10.90
Rel. List 1	6.80	6.66

Mean Number of Trials to Learn the Lists: Eleven Subjects, All Cycles (184 sessions).

TABLE II

	Condition A	Condition B
O.L. List 1	60.55	62.14
List 2	64.67	64.18
Rel. List 1	38.71	36.84

Mean Number of Errors to Learn the Lists: Eleven Subjects, All Cycles (184 sessions).

There is a positive correlation between the number of trials needed to learn the first list and to relearn it. For Condition A, the Pearson product moment correlation coefficient was found to be .35, and for Condition B, it was found to be .54. The number of trials, therefore, to relearn the list may in part at least be dependent upon the number of trials to learn the list originally. That in turn probably depends partly on the difficulty of the list. In order to avoid a constant error in having difficult lists always learned under either one of the conditions, five of the subjects

learned particular lists under conditions opposite to which the other six subjects learned them.

There was no reliable difference in the amount of overlearning [5] between Conditions A and B for any of the lists. (See Table III.)[6]

TABLE III

	Condition A	Condition B
O.L. List 1	84.95	80.99
List 2	73.83	71.67
Rel. List 1	78.78	91.75

Mean Overlearning Scores by Lists: All Eleven Subjects, All Cycles (184 sessions).

The tests considered to be crucial for differences in the two conditions were amount of recall, and savings in trials and errors. The amount of recall, however, was so slight under both conditions—most frequently zero—that the differences could not be considered very significant. The mean number of correct responses in the first trials of relearning (the recall score) for Condition A was .37 with a Probable Error of .59, and for Condition B, .59 with a P.E. of .72. The R, reliability of the difference found by dividing the difference between the mean scores by the probable error of the difference, was found to be .30 which is no difference at all.

Savings scores were calculated by the formula:

$$\text{Savings Score} = 100 - \frac{\text{Relearning Trials}}{\text{Learning Trials}} \times 100$$

Errors were substituted for trials in figuring the savings scores in the terms of errors.

The first cycle on the first six subjects showed a mean savings score in trials of 21.0 for Condition A and 39.3 for Condition B. (Table IV.) The savings score in terms of errors was comparable with 18.9 and 38.8 respectively. Four of the six subjects showed greater savings scores for Condition B in which they had been given the suggestion that they would be able to remember. One of these, Cob, had been refractory to suggestions of muscular contraction. Of the remaining two who showed less savings when told they would remember, one of them, Sam, had been amenable to previous suggestions.

[5] Overlearning was figured by the formula: $100 \times \frac{C}{T-C}$ where C is the number of correct units and T is the total number of units.

[6] The largest difference was for Rel. 1 and the R (reliability of a difference coefficient) for that is only .48, which is no significant difference.

TABLE IV

Subject	Condition A	Condition B
Cycle 1		
Cob	45.5	61.5
Ber	37.8	53.5
Rab	—31.0	46.5
Awd	14.8	10.0
Sam	37.0	28.3
Bri	22.3	36.3
Mean	21.0	39.3
Cycle 2		
Cob	53.0	43.8
Ber	27.0	44.0
Rab	24.0	32.5
Awd	37.0	15.0
Sam	22.0	13.3
Bri	6.3	45.5
Mean	28.2	32.3
Cycle 3		
Cob	63.3	44.0
Ber	47.0	32.5
Rab	—1.5	36.5
Awd	40.8	28.8
Sam	20.3	44.3
Bri	31.8	23.8
Mean	33.6	35.0

Saving in Trials: Six Subjects, Three Cycles.

With a repetition (see Table IV, Cycle 2) on these subjects, one of the four subjects who had tended to show positive results, Cob, had swung to the other side of the fence leaving three on each side, and a mean savings score in trials of 28.2 for Condition A against a score of 32.3 for B.

A further repetition (Table IV, Cycle 3) with these subjects showed still less consistency compared with the original findings. Subject, Sam, who had been negative in the two previous cycles now showed more than twice as large a savings score in trials for Condition B as for Condition A. Furthermore Subject, Ber, who had shown apparently positive results in the first two cycles now had a smaller savings score for Condition B than for Condition A. The mean savings score in trials for all six of the subjects for this cycle was almost identical for the two conditions, 33.6 for A and 35.0 for B.

One cycle run on five new subjects (Table V) gave a greater savings score for Condition B in only two cases out of five. The mean score was also slightly less for Condition B, 30.7 against 33.3. This did not indicate a difference, but merely meant that there were no positive results.

TABLE V

Subject	Condition A	Condition B
Hic	37.0	20.3
Wes	36.5	23.3
Orc	44.0	41.0
Eve	45.0	54.3
Qui	37.8	45.5
Mean	33.3	30.7

Saving in Trials: Five Subjects, One Cycle.

TABLE VI

	Condition A	Condition B
Saving in Trials		
Σ	2789	3296
Mean	30.3±24.1	35.8±19.9
Saving in Errors		
Σ	2559	3495
Mean	27.8±27.4	39.0±24.2

Mean and Probable Error of Saving Scores in Trials and Errors: All Subjects, All Cycles.

Taking all subjects for all cycles, there was no reliable difference between the savings in either trials or errors. In terms of trials, the mean for Condition A (Table VI) was 30.3 with a P.E. of 24.1, and for Condition B was 35.8 with a P.E. of 19.9. The reliability of a difference coefficient was $R = .18$. In terms of savings in errors, the mean for Condition A was 27.8 with a P.E. of 27.4, and for Condition B, 39.0 with a P.E. of 24.2. In this case, $R = .29$.

SUMMARY AND CONCLUSIONS

No consistent nor reliable differences were found between the two conditions in terms of number of trials or errors in learning the lists, overlearning scores, recall, or savings in terms of trials or errors. Under the conditions of this experiment then, it was found impossible to consistently affect the recall or amnesia for numbers recently learned even in subjects who responded readily to direct suggestions of muscular contraction. This is not surprising but altogether to be expected considering the relative infrequency of amnesia found in most normal adults who can readily be put into a light hypnotic trance in which suggestions of muscular contractions are accepted without hesitation.

NOTES

Duke University, Durham, N. C., announces a new psychological quarterly known as Character and Personality. Publication began in September, 1932, and will continue thereafter in the months of December, March, June and September. A British edition will also be issued simultaneously in London, and a German edition in Berlin. Annual subscription price is $2.00. The editor will be Robert Saudek (London) and among members of the editorial board, William McDougall, Alfred Adler, Pierre Janet, C. G. Jung, Johannes Lange.

THE CHICAGO SOCIETY FOR PERSONALITY STUDY

There has recently been organized in Chicago The Chicago Society for Personality Study, consisting of psychiatrists, psychologists, sociologists, educators, physiologists, and others, with the object of integrating the sciences in a scientific body devoted to the study of personality in all its phases, normal and abnormal, child and adult. Its membership is limited to 100. It contains the best students of personality problems and human behavior connected with various scientific and educational institutions in and around Chicago.

The officers elected are as follows: President, Dr. Meyer Solomon, Department of Neurology, Northwestern University Medical School; Vice-President, Professor Ernest W. Burgess, Department of Sociology, University of Chicago; Secretary-Treasurer, Dr. Paul L. Schroeder, Director, Institute for Juvenile Research.

The organization committee included, in addition to the above three, Dr. John Favill, Department of Neurology, Rush Medical College; Professor Robert H. Gault, Department of Psychology, Northwestern University; Professor Arthur J. Todd, Department of Sociology, Northwestern University; and Professor Harvey Carr, Department of Psychology, University of Chicago.

REVIEWS

METHODS IN SOCIAL SCIENCE. Stuart A. Rice, Editor. Chicago: University of Chicago Press, 1931. Pp. xii+822.

As everyone knows an unprecedented tidal wave of intellectual coöperation has surged through the American sea of social science. Its impetus has come from the beneficent but irresistible stirring of the Social Science Research Council, especially through its Committee on Scientific Method. It would seem that the crest of the wave has at last reached shore and deposited its treasure, a handsome and imposing volume describing the methods which have been successfully employed by the ablest navigators in the troubled waters of social science.

Since the fifty-two contributions to this large volume are critical summaries or reviews of as many standard pieces of research, the reviewer is faced with the unenviable task of preparing a digest of digests, and of writing a review of reviews. Even if he took only the 50 per cent of the contributions, which is approximately the proportion dealing directly with social psychology, the task would be tedious and the result stale. He should, however, at least mention by name some of the case-analyses so that the psychologist may appreciate the recognition given to his subject as a *social* science. There are reviews of Structural Psychology and the Psychology of *Gestalt* (R. M. Ogden), of Kretschmer's typology (H. Klüver), of McDougall's Methodological Concept of Instinct (C. P. Stone), of Thorndike's Measurements of Handwriting (J. Peterson), of the Prediction of Cultural Change (F. H. Allport and D. A. Hartman), of the work of the Judge Baker Foundation (F. N. House), of the Experimental Determination of Group Influences upon Mental Activity (L. L. Thurstone), and of the Experimental Study of Attitude, Meaning, and the Processes Antecedent to Action by Ach, and others in the Würzburg Laboratory (F. Fearing).

The editor explains that forty-five analyses were not completed or were rejected by the committee. The reviewer wonders whether this explanation accounts for certain conspicuous omissions from the volume, or whether the committee itself never sought to include them. There is no analysis of the epoch-making investigations of the Character Education Inquiry under the direction of Hartshorne and May, nor of the important psychophysical methods of Thurstone in the measurement of attitudes, nor of the method of *Verstehen,* nor of dialectical materialism in the newer Russian sociology, nor of *Wissenssoziologie.* Most serious of all seems to be the lack of incisive analyses of methods of studying *personality,* a concept sufficiently central and sufficiently obscure in social science to justify most careful attention.

Passing over the ever controversial question as to what a symposium should include, the reader will admire the volume as an editorial achieve-

ment. Professor Rice's account of the evolution of these case-analyses and his attempt to order them and to give a meaning to the term "method" are themselves contributions to social research. His own examination of the "semi-intuitive inquiry" by J. M. Williams on the Development of Rural Attitudes is a model of clear, compact, and radical analysis.

In a footnote at the end of Rice's analysis is to be found perhaps the kernel of the editorial point of view. Since it seems also to be the point of view of most of the analysts and therefore representative of American social science, it should receive a brief discussion. While realizing Professor Williams' difficulties in applying scientific techniques to the study of social life of past years in an American town, the analyst is nevertheless highly critical of the "concealed inferences" by means of which Professor Williams makes his interpretations. "Intuition" is the analyst's term of reproach. In the footnote in question he gives his opinion that sympathetic understanding of the object of investigation has no standing as a scientific method. What a pity that he did not include in the volume case-analyses of the frankly intuitive and interpretative methods of the German *Geisteswissenschaften!* Without permitting a hearing to the method of *Verstehen,* he should not assume that the simple processes of understanding and thorough acquaintance with the object of investigation have no merit as a scientific method.

One German reviewer of this volume (Professor Karl Mannheim, *Amer. Journ. Sociol.,* 1932, 38, 273–282) has further elaborated this criticism, and complained of the sterility of the "methodological asceticism" achieved by the editor and by the majority of the analysts. In Professor Mannheim's opinion their "exactitude complex" leaves most of the resources of the human intellect unemployed, and if pursued far enough creates for itself a "science of that which is not worth knowing".

Whether or not the reader regrets the omissions and disagrees with the point of emphasis, he finds that the volume rewards his patient reading. Its individual contributions are without exception instructive. The project as a whole is not only novel, but is a major experiment in coöperative labor and pooling of thought. The meaning of "method" has been clarified, the technique of case-analysis has been invented, and a striking new perspective has been achieved. Coöperative labor probably can go no further, at least at the present time. It will take a single mind to draw the threads together. It is to be hoped that the editor having accomplished the preliminary task so successfully will now synthesize the contributions, and publish his own conclusions respecting the methodology of social science.

GORDON W. ALLPORT.

Harvard University.

EXPERIMENTAL SOCIAL PSYCHOLOGY. By G. and L. B. Murphy. New York: Harper and Brothers, 1931. 709 pp.

No student of Psychology who wishes to make a thorough study of the social behavior of individuals can afford to overlook this book. It is an

excellent and inclusive survey of the results obtained by applying the experimental method to social behavior; it is both encyclopedic and critical. Together with the recent *Historical Introduction to Modern Psychology*, this book gives an excellent integrated statement of the 1931 brand of social psychology; it should be a boon to anyone wishing to synthesize the available data. No two modern books so far written offer so much so adequately.

The structure of the book is as follows: (1) basic principles: motives, individual differences, and the learning process; (2) the genetic study of social behavior: in children, and in adolescence; and (3) general laws of social interaction in our own society. To this are added excellent bibliographies in the form of chapter notes. These notes are packed with information and should not be neglected. Seven books are given special mention in an added bibliographical note. The index is suggestive: the Allports are cited 42 times, J. B. Watson 24, M. C. Jones 22, D. S. Thomas 22, frau Bühler and Hetzer 52 times, H. T. Moore 18, Woodworth 18, Goodenough 20—and Freud only three times, in passing. (These figures are cited as an interesting and reasonably fair index of stress.)

The purpose of the book is indicated in the preface: to show what the experimental method has yielded or may yield in the future to a "science" that is not as yet systematized. In planning the book over five years ago, a survey of the entire field of experimental social psychology was intended—when only a few hundred studies had been made. But studies were being published at the rate of about two hundred and fifty a year, and so the book was changed—for the better—to be representative rather than a manual that would soon be out-dated. The choice of data is open to question, of course, and the adequate statement of it is an even greater problem. Some will regard certain parts as of small practical value, or as being obscured by a statement of the many complex ramifications. Others will be intrigued by the critical comment, or by the inclusion of modern Russian studies which have not hitherto been generally available. It is especially true of this book that you may find in its format or content almost anything you wish; this is a compliment to its representativeness. This book, by very nature, will make its appeal to different readers for very different reasons, and it will probably be criticized from as many different points of view. For this reason no such evaluation of material will here be offered. (Cf. *Mental Hygiene*, 1931, 15, 860–862 for a review by F. L. Wells who writes with the clinical bias in mind.)

It is generally agreed by those with whom I have talked regarding this book that one of the outstanding chapters is that entitled "The Biology of Motives". This chapter states and evaluates the experimental data, and illustrates particularly well the way in which the notes may be used to illuminate the issue for those seeking a fuller picture than the text itself offers. Throughout the book one will find a comparison of techniques in historical order—to indicate the path of progress in methodology. Chapters are sometimes matched against each other in pairs for the sake of contrast, as a chapter on techniques being followed by one on the

conclusions—each relating to the same topic: the social behavior of children, for example. Or, again, the data on individual and group behavior are likewise held in contrast by similar balancing of chapters.

As to the book in general, my own reaction is one of admiration: the orderliness of arrangement, its inclusiveness, critical viewpoint, and insight into problems—all these make up a valuable reference to which I have already turned many times for data in easily available form. The format makes efficient use easy, once the content has been studied. The degree to which it will be used will depend on the reader who may use each study or reference, or only one; who can use the excellent notes extensively, or pass them by as beyond his purpose. In any event, here is a major contribution to psychological literature to be mastered before undertaking research in this or allied fields, or to be digested for an understanding of the trend of the times.

CHAUNCEY NEWELL ALLEN.

PERSONALITY AND WILL. By Francis Aveling. New York: D. Appleton and Company, 1931. Pp. xi+246.

The theory of personality and will set forth in this book is rather elaborate and not so clear. Although based, in many respects, on experimental data, a number of highly controversial premises are involved.

Aveling dogmatically turns to a sort of interactionism between body and soul, to levels of consciousness and unconsciousness. In discussing the question of psychological freedom, he looks askance upon experimental psychologists who "as a rule carefully avoid this problem and make no pretense of attempting to solve it". His numerous basic assumptions include the following notions:

(1) There is a basic self-identity which remains unchanged through various normal and abnormal variations.

(2) Experience points to consciousness and existence; immediate intuition reveals consciousness.

(3) The soul is "an energy or substance-activity in relation to other substance-activities or energies, as that which determines them to the particular configuration of relationship in which they stand and energize" (188).

(4) The only solution for the problem of free-will can be found by appeal to experience.

(5) Of the will-processes, *striving, performing, carrying-out,* are activities on a mechanical plane; *resolving, deciding, choosing,* however, are capricious and non-deterministic activities.

(6) Concepts of a highest good are set up by individuals. These concepts are values making possible free resolution and free choice.

(7) Willing is purposive, anticipatory of consequences.

(8) Orientation towards goals is evidence of conscious as well as of "unreflective, semi-conscious, or even of totally unconscious will".

Aveling welds these variegated hypotheses together and derives the following conclusions: "the intrinsic principle of personality is simply

intelligent will" (241); "Personality, accordingly, may now be defined, not merely as the organization of all so-called mental contents, traits, capacities, and reaction tendencies, but as that final perfection which virtually includes in itself all the principles of human activity and constitutes a unitary, self-conscious, and (to some extent at least) self-determining individual" (214); "In the strictest meaning of the term, then, a person is an individual incommunicably existing in himself, who is not merely will, or energy elicited by goals and determined by motives, but an intelligent will contemplating means to ends and making its own motives" (242).

NATHAN ISRAELI.

New York City.

VISAGES ET CARACTERES. Etudes de Physiognomonie. By Louis Corman, with the Collaboration of Gervais Rousseau. Preface by Doctor Vurpas, Physician of the Salpêtrière. 319 pages. Paris: Plon. 1932.

This book is very timely now when the study of morphology and its characterological correlates is taking an ever greater place in psychological and psychiatric thinking and practice. But it represents a viewpoint very different from those of Viola, Kretschmer, Draper and other contemporary typologists. It is revival of the physiognomy and typology of the ancients—chiefly Hippocrates and Aristotle—on a plane quite distinct from the usual analytic and quantitative anthropometric studies. The author insists on the insufficiency of these methods to appreciate the *value* of the *living* human form, which is the proper subject matter of physiognomy. "To reduce the forms to their numerical expression is at first sight seducing. But what shall we measure in this face? Vertical and tranversal diameters, lengths, circumferences? That is what is usually done, but it gives only a feeble notion of the forms to be studied. Shall we then measure volumes, dihedra, curves? . . . that becomes very difficult if not impracticable. Let us live a biometrist's dream: we have measured everything in these features, we can supply their mathematical reconstruction. All they contain of *quantity* is in our hands. Yes, but we have not given these features life, we have not rendered their *quality*."

The style is clear, the thinking logical, yet the book is quite readable. Corman takes great pains to define his terms, to state his point of view unmistakably—a practice one could wish more widespread in psychological works.

He sets out to study certain of the "morpho-psychic couples" of man, limiting himself neither to the purely static or anatomist's form nor exclusively to the form in action, the expression, *la mimique*. He studies all that can be discovered about a human being by the unaided senses of sight and touch, without any artificial division. Indeed, the analytic parts of the work, where each trait is considered separately, are deliberately used only for didactic purposes, and each one is immediately followed by a synthetic chapter.

In the first part of the book Corman studies the general form of the body, without neglecting the complexion, the tension or flaccidity of the tissues. The hand and the head, principally the face, are studied at length as the most revealing features. Here he follows the method of Sigaud, with its division of the face into three stories: buccal, nasomalar and oculofrontal, corresponding respectively to the three "vestibules": digestive, respiratory and cerebral. Different types are established according to the predominance of the different variants of each of these traits in the total "mosaic".

The second part is a brief but systematic review of the components of character (in the broadest sense): mood, with its two modalities euphoria-dysphoria, irritability-phlegm, tendencies, spontaneous activity, affectivity, intelligence, and of the types of resultant character mosaics.

The third and fourth parts are devoted to the correlation between morphological and psychological traits and types. There is an analysis of the morphological correlates of each component of character, followed by a synthesis, "an assemblage of the elementary forms, corresponding to the joined elements of character", with an attempt at estimating the value and the efficiency of each character type. Eight simple morpho-psychic types and their alloys are isolated.

The fifth part "Physiognomic practice" considers the value of different means of observation, the qualities of the observer, examples of physiognomic studies of different types. The sixth treats of possible applications toward making the best use of individual aptitudes, as in the choice of types of schooling and "moral environment" for children, vocational guidance and, in general, a better understanding of other men and even of oneself.

The author's attitude is everywhere one of humility before his material, which he believes to be essentially qualitative. He therefore makes no effort at forcing it into the moulds of quantitative science. Likewise he draws no conclusion as to the nature of the relations between form and psyche. "In truth, the psychic fact does not depend on the morphological fact more than the latter is determined by the psychic fact . . . nothing authorizes us to postulate causes, effects, dependencies. . . . The psychic and the morphological fact are both the expression of . . . living activity in its deepest reality."

In his psychology he is behavioristic in his method, without falling into the doctrinal extravagances of behaviorism any more than into those of psychology viewed as the pure study of "consciousness". His psychology is above all a value psychology, which, with its rebellion against analytic and quantitative methods, is similar to the points of view of emergence and *Gestalt* psychology. Curiously enough, the French word *visage* includes more than the purely anatomical term *face*. It cannot be exactly translated into English, but corresponds most nearly to the German *Gestalt*. (Features, taken collectively, is an approximation.)

The book is illustrated with schemas which could, perhaps, be advantageously replaced by photographs. It would, further, be interesting if

more examples drawn from real cases, particularly psychiatric cases, had been included. It can be hoped that Corman will do this in a future work, as he has had the unique opportunity of having at his disposal the rich material of Doctor Vurpas' psychiatric service at the Salpêtrière.

A. F. Liber.

Les maladies de l'esprit et leur médecins du XVI^e^ au XIX^e^ siècle. Les étapes de la connaissance psychiatrique de la Renaissance à Pinel. By M. Laignel-Lavastine (Professor at the Faculty of Medicine of Paris) and Jean Vinchon. 377 pp., illustrated with full-page engravings. Paris: Maloine, 1931.

A critical and impartial examination of history always yields a mixed feeling of encouragement and discouragement. This book follows the rule. One is occasionally discouraged, while reading it, on finding how old are so many of our "modern" views and discoveries in psychopathology. Encouraging is the tremendous progress in the treatment of the psychopath: the various measures taken both for his protection and that of society, the humanization of the attitude toward madness, the gradual liberation of psychiatry from animistic beliefs and magic-tainted practices, the ability of modern techniques to apply, at least occasionally, a radical treatment to some of those cases where the etiology has come to be known. Less encouraging is the record of progress in the knowledge of causes of mental ills. The assurance of views on incubi, witchcraft, possession by demons and movements of animal spirits remind one all too strongly of certain contemporary theories, like those of intra-psychic structure and metapsychology, or, on the other hand, the grosser physico-chemical explanations like reflexology, which, alike, explain everything and are set forth as if they were proven fact.

The book is full of material interesting not only to the psychopathologist. It is also a significant chapter in the history of modern thought—one somewhat neglected by most of its interpreters. Particularly well adapted to the task is the spirit of humanism combined with the broad historical as well as psychological erudition of the authors.

There is more material than can be reviewed in detail. The authors present a series of essays, each representing a period or a tendency, from John Schenck (1530–1598), still slavishly parroting Galen, to Pinel, including Ambroise Paré, Garzoni, Thomas Willis, Etmuller and Isbrand of Diemerbroeck, Paul Zacchias, La Mettrie, Locke, Condillac and their influence on the Encyclopedists, Lavater and temperaments, Mason-Cox and his rotating machine, the precursors of Bayle. Among the best morsels are: the observation of an incubus in the 17th century, given in full, with the cure, accomplished in very modern fashion by persuasion, the patient having understood the nature of his illness—the first doubts on the supernatural origin of incubus, expressed by the exorcist Midder, who occasionally called physicians to help him—the comparison of symbols in the 16th century to those of Freud and Jung—Thomas Willis and his often startlingly modern ideas on the rôle of heredity and of the vegetative

nervous system in mental disease, his surprisingly accurate description of the disturbances accompanying intracranial hypertension in tumors and abscesses of the brain, his insight into the connection between mania and melancholia: "These two affections are so similar that one often transforms itself into the other, and that one often ends in the other. . . . Often, these two diseases succeed one another and the one makes way for the other like smoke and flame."

Paul Zacchias, medical expert at the court of the Rota in the Papal States (1584–1659), was the originator of forensic psychiatry. One finds a classification of the degrees of mental defect by mental age similar to that now in use, a shrewd treatment of the irresponsibility of an epileptic in an absence, a study of the problem of simulation of psychopathic states. To the question "If the intelligence of demoniacs is alienated by the demon, can one rightfully consider them insensate?", his answer, delicate in the shadow of the Inquisition, was that the demon could only install himself in individuals affected with a natural state depending on the realm of medicine.

The work of Pinel is treated at some length. Some little known facts about him are given. One's admiration for Pinel cannot but be enhanced by the recital of his initial failures, of the difficulties he met and overcame in the midst of a strife-torn world. Odd is the story of Berbiguier, one of Pinel's patients, a hallucinated paranoiac with delusions of persecution, who wrote into his memoires ("Les Farfadets, ou Tous les Demons ne Sont pas de le l'Autre Monde") all his delusions, in which Pinel played the central part as persecutor, "now under his human form, now invisible".

The book is handsomely gotten up and illustrated with full-page engravings, some very curious, as Ludovico Dolce's diagram of brain localizations (16th century), worthy of his more recent descendants stigmatized by Head as "schema-makers"; the reproduction of one of Mesmer's advertisements in a newspaper, the visions of Berbiguier.

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